FIFRA SCIENTIFIC ADVISORY PANEL (SAP)

OPEN MEETING

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FUMIGANT BYSTANDER EXPOSURE MODEL REVIEW: PROBABILISTIC EXPOSURE AND RISK MODEL FOR FUMIGANTS (PERFUM) USING IODOMETHANE AS A CASE STUDY

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Reported by: Frances M. Freeman, Stenographer

C O N T E N T S

DR. ROBERTS: If I can get the panel members to take their seats. Let's restart the meeting.

In case there are members of the audience that were not here for yesterday's session, I think it would be useful for us to briefly reintroduce the panel.

Let me ask, again, our panel members starting on my left to state their name, affiliation and the expertise that they bring to the panel's discussions today.

DR. HEERINGA: Good morning. I'm Steve Heeringa. I'm the Director of the Statistical Design Group and research scientist at the University of Michigan, Institute for Social Research.

I'm a biostatistician and my area of specialty is designs for population based research.

DR. PORTIER: I'm Ken Portier, statistician and associate professor at the University of Florida, Institute of Food and Agricultural Sciences. I work in the area of environmental risk and probabilistic risk assessment.

DR. HANNA: Good morning. I am Adel Hanna, I'm associate professor at the University of North Carolina.

My area of expertise is air quality modeling and meteorological analyst.

DR. SHOKES: Good morning. I'm Fred Shokes. I'm the professor of plant pathology, I'm a practical guy, at Virginia Tech. I work at the Tidewater Agricultural Research and Extension Center in Suffolk. I happen to be the director there.

DR. MAXWELL: Good morning. I'm Dave Maxwell at the National Park Service in Denver. My areas of expertise are air quality monitoring, permitting and air dispersion modeling. I have a meteorology background.

DR. WANG: Dong Wang from the University of Minnesota, I'm associate professor of environmental biophysics, specialized in the fate and transport of environmental contaminants, pesticides, fumigants.

DR. WINEGAR: Eric Winegar, principal of Applied Measurement Science. My background is monitoring and measurements, analytical chemistry and exposure assessment.

DR. OU: Li-Tse Ou. I'm a scientist with the University of Florida. My special area is the fate of pesticide in soil. I'm a soil microbiologist.

DR. SMALL: Mitchell Small. I'm in the departments of civil and environmental engineering and engineering and public policy at Carnegie Mellon University in Pittsburgh. I work in the areas of environmental modeling and statistics.

DR. MAJEWSKI: I'm Michael Majewski. I'm a research chemist with the U.S. Geological Survey. My background is in developing methods to measure and estimate post application volatilization of pesticides and also atmospheric transport and fate of organic chemicals.

DR. BAKER: I'm Dan Baker with Shell Global Solutions in Houston. I work on emissions modeling and air quality modeling.

DR. BARTLETT: Paul Bartlett, City University New York. I work in the area of air transport, environmental fate modeling, emissions monitoring, measurements.

DR. SPICER: Tom Spicer, professor and head of chemical engineering at the University of Arkansas. My field of expertise is atmospheric dispersion.

DR. YATES: I'm Scott Yates, interim research leader of the Soil Physics and Pesticides Research Unit, USDA/ARS, in Riverside, California. The area of research -- my research interests are environmental fate and transport of pesticides in soils and volatilization into the atmosphere.

DR. ROBERTS: I'm Steve Roberts. I'm a professor, toxicologist at the University of Florida with joint appointments in the Colleges of Medicine and College of Veterinary Medicine.

DR. SEIBER: I came in a little late. Jim Seiber. I'm with the USDA Agricultural Research Service in Albany, California. And before that, I was at the University of California, Davis, and University of Nevada, Reno, working in the area of experimental design for pesticide environmental fate studies.

DR. ROBERTS: As we begin our meeting, there are some important announcements from our designated federal official, Ms. Myrta Christian. Ms. Christian?

MS. CHRISTIAN: Thank you, Dr. Roberts. I really don't have any extra announcements. But I just want to say that I'm looking forward to another day filled with lively discussions and great participation by the panel. Thank you.

DR. ROBERTS: We have as the first thing on our agenda this morning a follow-up on previous day's discussion by Mr. Dawson. Did you want to make some remarks as a follow-up to yesterday or should we get into our questions?

MR. DAWSON: I just wanted to thank the panel for a very thoughtful discussion yesterday and look forward to more of the same.

I would also like to introduce Mike Metzger, who is a branch chief in the Health Effects Division. He will be up at the table with me. Margaret will be here, I'm told, momentarily.

DR. ROBERTS: Thank you. As I recall, we left off completing question number three, which brings us to question four.

Before we start our discussion today, again, I would like to remind the panel that the acoustical situation in here is not great.

It will really help out if when you make your comments if you could pull the microphone in close and speak directly into the microphone. I think that will really help in terms of being able to be heard not only around the table here, but also by the people in the audience.

Let's go to question number four.

MR. DAWSON: Question 4, this one has to do with our general theme of system design and input.

The integration of actual time-based meteorological data into ISCST3 is one of the key components that separates PERFUM methodology from that being employed by the Agency in its current assessment.

There are several potential sources of these data including the National Weather Service, Federal Aviation Administration, California Irrigation Management Information System or CIMIS, and the Florida Automated Weather Network or FAWN.

The Agency is also aware that there are several approaches that can be used to process meteorological data and acknowledges that PERFUM used PCRAMMET which is a standard Agency tool for this purpose as well as other techniques in some cases (for example with the FAWN and CIMIS data).

Various data sets from both California and Florida were used as the basis for the PERFUM case study. Please comment on the methods used to select the monitoring station locations. What criteria should be used to identify meteorological regions for analysis and how should specific monitoring data be selected from within each region?

Please comment on the manner that data from the selected various stations were processed. Data quality and uncertainty associated with these data vary with the source. Does the panel agree with the approaches used to characterize these factors?

Anemometer sampling height has been identified as a concern by the Agency in preparation for this meeting. What are the potential impacts of using data collected with different anemometer heights and analysis of this nature?

Does PERFUM treat stability class inputs appropriately? Does PERFUM appropriately calculate bounding air concentration estimates by concurrently using upper-bound meteorological and emission/flux inputs?

DR. ROBERTS: Thank you.

Dr. Hanna, could you lead off our discussion in response to these questions?

DR. HANNA: Thank you. This is Adel Hanna, University of North Carolina.

I'll start by the first part of question four which is, please comment on the methods used to select monitoring station locations, what criteria should be used to identify meteorological regions for analysis and how should the specific monitoring data be selected from within each region.

This is a kind of question that really implies on regional meteorological patterns and local features or micrometeorology associated with different small scale or rural areas.

So selecting the monitoring station first, as was done in this study, we look at where is the coastal station or near water stations.

And this is an important part of the selection, because really coastal stations have a different stability criteria and also different wind patterns. The coastal stations are different from inland stations, for example, has the land, sea breeze phenomena basics (ph) as a result of the difference in temperature between the sea water and land.

So there is a reverse in the flow, wind flow between day and night. So that's important to be accounted for during a study that really looks at a dispersion of plume in agricultural field or something like that.

So it has been done in this study. I think there was a number of stations or two stations that were very close to the coast. And the other thing that we would like to look at is the terrain effects or places with different topographies.

Those also have different meteorological features from flat surfaces as was done in this study, so areas or fields linked near terrain or near high elevations.

There also could be some phenomena related to the wind directions between different -- especially in different seasons or something called chinook or other winds.

Again, changes in the wind direction and the speed too, which affect the dispersion and the concentration associated with when there is that kind of application being applied to a certain agriculture field.

On a more regional structure, of course we look at in order to classify different regions or have certain categories of certain regions, we look really to the -- at least from a meteorological pattern, the precipitation pattern, because it is linked to the clouds in general as we know.

And the cloud is one of the important parameters that we use in the ISCST3 model to decide on the stability, which again, is another important key to the calculation of the dispersion.

We look at the temperature field. And we look at the terrain, as I mentioned, what are the specifics of even the nature of the land soil or agricultural field on this different region.

So those are all factors that can help us decide. At least -- is a certain region -- when accounting a certain region, is it representative? If we can in some kind of confidence assume that the data collected at this region can be applied or the analysis used in this region can be applied in different parts of this region with some kind of confidence in this.

It might be different from application at other locations with different meteorological characteristics, but that, at least, will help us in categorizing or dividing the country into several regions with specific meteorological characters.

When we look at the processing of the data in this study, I think the data processing was done in an accurate way according to the ISCST3 standards or rules.

Still the question of missing data -- I was not clear if missing data was used. At certain parts of the document it said it was not used and another part of it said it was used.

With the missing data, when you fill the gaps with missing data, as was mentioned in the report, the EPA recommendation is really to use the data within few hours of the missing period. So if this few hours is like two, three hours or so, that might be an acceptable procedures.

On the other hand, if -- still one of the key parameters that cannot be interpolated or -- yes, it can be interpolated but would not be interpolated with the high accuracy is the cloud parameter. Usually, clouds are highly variable between hour to hour in most cases.

And interpolation into that cloud cover might also lead to certain errors. But as was mentioned in the report, the number of missing data was not very significant.

But this is something that we have to keep in mind as we are discussing general rules that the cloud cover, especially temperature, might be easy to interpolate -- and wind to a certain extent.

But the cloud cover, which is, again, a key parameter when we are looking at the -- where we calculate stability. So the cloud is not (ph) a trivial things for the missing data.

Also, in this analysis as was mentioned by Dr.

Reiss yesterday, that the number -- there are three -- four observational systems. One of them is the National Weather Service data, which has the most kind of accurate and has the quality control applied to it -- and other data from California and Florida and the Federal Aviation.

But in these data really, as was shown, the National Weather Service data --and I think the Federal Aviation data has a cloud cover on it. And that and the other data sets from California or Florida I believe did not include cloud cover.

As I mentioned, cloud cover is needed to calculate the stability index in the ISCST3 model.

So what was done was just to do a kind of different approach to calculate stability index. At the end, they looked in a general, close to each other, but I think from a method of consistency it was preferable to use the same kind of approach through the whole study.

And of course, the other point of concern is the lack of quality control, I think, and quality assurance that was applied to the data, I think, from Florida. And that by itself can create a lot of noise during the calculation.

So I really would prefer to use, in general, the National Weather Service data, but as was mentioned also yesterday it is mainly in the urban or large airport areas. But still, the quality of the data is very highly -- are very high compared to the other data sources.

So I would say the National Weather Service data is a really -- is the real source of this information.

I would also like to suggest other data sources that can be acquired. I think and I know that some state climate offices really have a good collection of data if we want to kind of generalize this method or apply it or apply PERFUM at different states or different regions.

For example, I know that the North Carolina State Climate Office has something called the CRONOS Network which is a database for the weather information for about 216 stations, which includes the National Weather Service data, but also include stations run by the state climate office similar to the way being run by the National Weather Service.

And actually, the state climate office takes special attention to agriculture needs in the design of the additional stations from the National Weather Service data.

So I know that other states, although I cannot say that every state, might have this observation or weather observation networks that can be used for certain applications.

By the way, the CRONOS Network includes data and measurements from South Carolina, Georgia, Virginia and Tennessee. So this kind of information can be looked at in accessing information related to the agriculture application.

But again, the main point is really to identify if even we are using data from different sources. We also need to identify the biases and the errors in each of these data sets in order to provide a good estimation of the or quantification of the uncertainty when we really run PERFUM.

Another source of data, which really does not measure the weather observation per se, is something called the SCAN, which is Soil Climate Analysis Network. These stations are spread all over the United States. There is no SCAN information in California, but there are in Florida, for example.

They are focused on the agricultural areas of the United States. And they are maintained again by National Resources Conservation Service, and they are mainly used for monitor of draught.

But the point that I'm trying to make here is that since they are targeting agricultural areas for agricultural need, they may also be a good candidate if a kind of mobile weather station can be implemented within the sites. And that, of course, needs some communications between EPA or contacts between EPA and the Agency.

This is another alternative that really compliments the weather needs for the dispersion models.

And then the other source of information that I would also recommend and I mentioned that yesterday, that when there are -- really there is no adequate weather observation at certain areas. There are a number of modeling runs like the Colorado State TRAMS (ph) model or the Encar (ph) MM5 model that even EPA now have an archive for, say, years like 2001, 2000 -- start to have a full year of model run all over the United States.

The data from this model, of course, is the same kind of information of weather data like the temperatures, wind pattern and profile, even mixing high and all the stability.

What I'm trying to say is that these sources from these modeling runs can subsidize the meteorological information when there is no network or measurements available and can be used really for scanning and screening.

And even I would go further, they can be used even for comparison at certain areas between the results of PERFUM using the observational data and the data from the models, for example.

I think I went over a number of items in my response that follows even the questions that were read.

The last point that I want to go over is what are the potential impacts of using the data collecting with different anemometer heights in an analysis of this nature.

This, as we heard, that there were some observational data that was used, yesterday, was the anemometer levels where we measure the winds where the general recommendation is at 10 meters height and can run between 6 and 10.

But there is one specific data set, I think the California data set, was the anemometer was at two meter. So basically there was the concern between what is the difference between the two meter winds and the 10 meter winds.

And in responding to this, I think we are talking about the boundary layer in general, but actually we are talking about something we call the surface layer, which is the lowest 10 percent of the boundary layers.

The layer which is impacted is the surface, land surface itself. So if we say that the boundary layer in general during daytime runs to one kilometer high to two kilometer high, we're talking about the lowest 100 meters and changes that goes near in this 100 meter, but now we're talking about the 10 meter change.

And what we know that the -- of course, is apart of -- going away from the surface, wind speed picks up rapidly to an -- I wouldn't say rapidly, but start to accelerate until it comes into certain levels.

So there is a difference between the two meter wind measurements and the 10 meter wind measurement. In general, the 10 meter difference should be a little bit higher. But the surface wind varies a lot and varies with a certain -- in direction and in speed. We are looking at that.

And that's the idea of putting it at 10 meter, actually, is to try to get away on the surface, what is the friction and other surface effects that affect the measurement in a way that make it to be less certain than the 10 meter height.

Picking over that in the daytime versus nighttime pattern in the boundary layer, this one kilometer or less during the night has different really characteristics. For example, it can have during the daytime over land, for example, during daytime over land the wind profile in a clear weather --

typically, in a clear weather day, typically, have very little speed or change in the direction within the boundary area.

Why is this happening? This is as a result of what we call the turbulent eddies. The mixing is taking place in the boundary layer during the daytime. So basically there is homogeneity in the structure of the wind and in the direction and speed. That's why we say for example it is the mixing layer.

But still, within the first few meters, which is in the surface area, this wind speed will change between the 2 meter and the 10 meter.

On the other hand, during the stable boundary layer, which we confounded of course over night or over land -- on any surface where there is the colder, the surface is colder than the overlying air, the stable boundary there is characterized by less mixing than what we see, what I mentioned just mentioned in the -- it was the kind of unstable daytime boundary layer.

In this case even the wind speeds increase in the boundary layer until they reach a certain -- the top of the boundary layer itself.

So in answering, again summarizing my answer to this 2 meter, 10 meter question, yes, there is difference between the 2 meter and 10 meter measurements. Although within the kind of variability that we see in the surface wind in general, it is a very variable parameter, it might not be recognized.

But it is my opinion that it is preferable to have data at the standard heights, at the same heights, in order to do the comparison adequately.

And I moved to my last part here with the -- basically, is how we account for uncertainty. As I also mentioned yesterday, there is a way also to include the -- to test the uncertainty or to include uncertainty in the model parameter like the horizontal and vertical dispersion, as I discussed with Dr. Reiss during the panel meeting yesterday, that it is possible really to account for the stochastic end biases in this parameter based on separate information and include them in the model simulation as a model parameter other than as an input.

The idea that you are supposed to have the perfect inputs, no errors in the winds, no errors in the stability parameters and everything, but still there will be certain sources of uncertainty related to the algorithm method used in the ISCST3 model.

And this also can be accounted for if we introduce certain parameters. Mostly these biases are in the log normal distribution forms and can be used, as I said, as relating to the stochastical biases and can be multiplied with the dispersion parameter.

I will stop here.

DR. ROBERTS: Thank you, Dr. Hanna, for those comments. Let's now go to Dr. Bartlett. Are there other comments or areas of agreement or disagreement that you want to highlight with Dr. Hanna's comments?

DR. BARTLETT: Yes, I can be brief, because Dr. Hanna was very comprehensive.

I previously asked about the terrain topographic issues, yesterday. And I believe the answer was that all the areas were relatively flat, so that the terrain features were not an issue in these sample sites. But that may not always be true. So the generalization problem is there.

Also, it may make sense to -- of course, the problems of having a comprehensive study and sample size, to know how that would be affecting buffer zones in other typical farmland situations which would affect the behavior of the winds under 10 meters.

So I think that's part of an issue here, is what is going on in those first 10 meters and how easy it is to model that and how that might be affecting the winds in that short distance and the boundary in finding the buffer zones.

The issue on processing the data and the other sources, it is not trivial to bring in new data sources. So I think that's commendable to bring in other types of weather data and process that and work with that.

But I think the data quality control problems are real. I was wondering if you had any warning routines to spot like negative winds and things like that?

DR. REISS: I did, particularly with the FAWN data. There were checks within the processing program that we developed to make sure -- basically, to detect anomalous values. And we did find a number of them with the FAWN data. It took -- basically, we had to eliminate a lot of data and do more interpolation to account for that.

DR. BARTLETT: I guess that brings another thing, a clarification that I need. You had said when there is missing data, in at least one source, that you left it out because of the problems of the degree of work that would take to fill that in.

In some sense, that makes sense. You have a sample over five years to leave it out as opposed -- and then work with that data as opposed to possibly skew.

DR. REISS: I agree with that. The EPA guidance when you do a permitting application or any kind of dispersion modeling application is to have a one hundred percent complete data set. And they have stipulated various rules that we tried to follow to make that data set complete.

My own personal opinion is if you have one or two percent missing data, the more accurate thing would just be to leave it out and run the model with the 98 or

99 percent of the data that you have.

As to the one data set where there was some missing data in the file, that was the CIMIS data, was provided to us from the state of California.

They processed it and filled in the missing data where it was convenient and accurate to do so and chose not to do so for the one percent or so of the data where it would have been very difficult to do so.

So I just felt that the appropriate thing was just to keep that data set the way it was.

DR. BARTLETT: I believe you are using the standard processing to create the stability classes. So you are within the guidelines there.

But overall, it is -- to me, having a dynamic vertical wind mixing parameter is problematic, but in the -- but that's inherent in ISC and the approach there, which may be causing some of the variation that you are having in the model.

The CIMIS -- I don't know if it is related as well, is that your measurement stations are at -- they were at one and a half meters for your monitoring as well?

DR. REISS: For the flux monitoring, yes. And when we ran the ISC for the back-calculation, we would code whatever the monitoring height was into the model. So the model will predict the concentration as a function of height.

DR. BARTLETT: In one instance you did have a 10 meter wind station as well when you did the more direct measurements?

DR. REISS: The wind stations on the sites varied in height. And there were several where we had both 2 and 10 meter measurements to -- this is the meteorological stations, and we did that to investigate this issue of 2 and 10 meters.

And we saw very little -- we calculated the flux with both sets of data and saw very little difference and no real apparent bias.

I agree that the winds are probably -- they are generally lower at two meters, but within the experimental variability that you are observing, that just wasn't apparent.

DR. BARTLETT: That appears to me that that will be a problem of generalization of the model. Using the weather data, standard weather data for application of something emitting at zero is always difficult.

So when any terrain starts to become a factor, I'm not sure about the applicability of the buffer zone. I mean it is just an uncertainty in the process.

DR. REISS: I agree that we need to probably look at the uncertainty associated with terrain impacts.

There could be a lot of scenarios that are possible. And there could be a lot of situations where the terrain just increases the dispersion of the pollutant. We'll have to look at that in more detail and report back.

DR. HANNA: I think with terrain, Dr. Reiss says the AERMOD might be actually handling the terrain in a better way than what is in the ISCST3.

So that might be at least if he is looking at the study of the AERMOD. The terrain is one of the characters or the parameters that would be highly improved in the AERMOD, as I understand.

DR. REISS: I agree. We will eventually, if this goes forward, and AERMOD is approved, which we expect, we have expected for some time now, it has been delayed, that AERMOD would be model of the future to incorporate into PERFUM.

DR. ROBERTS: Dr. Majewski?

DR. MAJEWSKI: The methods for selecting the meteorological monitoring sites locations, you mentioned that the stations were chosen to be most representative of the agricultural growing area. Yet in the document on Page 66 that describes for the model, it says that you recognize that there weren't enough met stations to draw any broad conclusions.

But the conclusions you did come up with was that there was no significant difference between the NWS, ASOS and CIMIS data and also that there was no discernible pattern between coastal and inland stations or ag and urban stations.

I think there needs to be a hierarchy decisionmaking about what or where the met station data you are using and that should be near the application location. Because I think if you don't have a clear decisionmaking step process, you might be able -- somebody might look at or use data from a coastal met station with this statement here that there is no significant difference. I think it might lead to a problem.

But is this no significant difference in the early --

DR. REISS: It is in the five year result.

DR. MAJEWSKI: Five year results?

DR. REISS: As a general point, when the buffer zone tables are ultimately developed, there may be just one number for a national buffer zone. There could be numbers for different regions. That's really not decided yet.

But when you look at these -- I think the goal when you are looking at the meteorological data and choosing what stations to use, it is not to get the right location of the maximum concentration for the inland valley or Santa Barbara or getting that directional impact right.

The real goal, I think, is to use a number of stations that characterizes the variability that you could observe in the environment.

One of the key variables, and probably the driving variable, is the standard deviation of the wind direction. You want to use a lot of stations, look at a lot of stations that vary that variable and try to decide that.

But ultimately, we're going to have to reduce this considerably, distill this considerably among regions to come up with some national buffer zone strategy. California, we might be able to do something different for California, specifically.

But talking about using a station close to the application, it is really not the way the model is ultimately going to be used. It is going to be used to generalize across regions and across states.

DR. MAJEWSKI: So that's not your problem. That's the Agency's problem. Right?

DR. REISS: It will be my problem too, I think. But I think it is like one of those things, it is part science and part policy as to how to actually decide on what to do about that.

DR. MAJEWSKI: Then I guess that answers the next question, what criteria should be used to identify meteorological regions.

Should you be using the met data that's nearest to the application areas? And also, the National Weather Service data seems to be the most complete, the most standardized stations and have the best quality control associated with it.

So I think wherever possible, the National Weather Service Met data should be used, and then CIMIS in California used with some qualifications, I guess.

I guess the uncertainty would be larger with the CIMIS data.

Moving on to the manner in which the data was processed, in section 4.33, it says that California DPR used the EPA recommended factors to adjust the sigma theta method for data collected at other -- 10 meters. However, the wind speed and wind direction were not adjusted for the lower measurement height. I'm not exactly sure what that means.

DR. REISS: There is a variety, a table, basically, where when you use the sigma theta method you look up the wind speed. They ask you, it stipulates that you want to adjust that wind speed to 10 meters. And then you look at the adjusted wind speed to 10 meters to get the stability class.

So they adjusted it to do the stability class calculation, but when they actually processed the ISC file, they used the actual measured wind speed. I think that was probably the best decision. There is quite a lot of uncertainty in adjusting from 2 to 10 meters.

Actually, what I have observed is that if you look at the formulas to do that, you get a -- the formulas predict that there is a very, very large difference for nighttime concentrations between 2 and 10 meters. I mean, it can increase by a factor of two for like E and F (ph) stability.

We have compared CIMIS data with National Weather Service data for stations that are close to one another. And we also have two studies where we have sets of 2 meter and 10 meter data that were collected concurrently.

I think the formula fails, it seems to fail. The difference just isn't that great during the nighttime. So it's another nice kind of data set that we have out of this work that I think we can publish and possibly make some recommendations.

I would note that the ISC model doesn't make any adjustment between 2 and 10 meters. The anemometer height is an input to the model, but below 10 meters it doesn't do anything with that variable. It is only if you add -- if it is above 10 meters that the model will actually make an adjustment to the vertical profile of wind.

So I think people recognize that there is some uncertainty in trying to extrapolate winds down that low. And the data we have might be a little helpful for that.

DR. MAJEWSKI: I guess that leads into the last question. The potential impact of different anemometer heights.

Obviously, it has been mentioned, the wind direction is more variable down the lower it is and it increases the uncertainty in the buffer zone estimate. I guess it is another reason to try and use the National Weather Service data first because of all the factors associated with those sites.

DR. ROBERTS: Thank you, Dr. Majewski. Dr. Maxwell, points to add?

DR. MAXWELL: Dave Maxwell, National Park Service. If you look at that question, there is about seven or eight different subquestions within that. I'll try to cover this briefly and reinforce some of the statements that my colleagues have already stated.

If you look at the first one, the methods on selecting the monitoring station locations, have you ever thought of using a portable meteorology tower to do your studies?

DR. REISS: We do. All of the flux studies had an on-site meteorological tower. I didn't make that clear. That was the case. And in two of the studies we had two towers, one at 2 meters and one at 10.

DR. MAXWELL: Another issue brought up was perhaps looking at the state agricultural weather stations. They may not be the most (inaudible) when we agree that the National Weather Service sites are probably superior, but they may have some local data that could support, perhaps, missing time periods in some of your model runs.

I imagine California would probably have their own summary of state agricultural weather stations.

DR. REISS: It is the CIMIS Network, which we use.

DR. MAXWELL: It is only the CIMIS Network?

DR. REISS: As far as I'm aware.

DR. MAXWELL: That's it?

DR. REISS: Yes.

DR. MAXWELL: Okay. How many sites are there across the state?

DR. REISS: I don't know the exact number, but there are dozens. There are many.

DR. MAXWELL: Fine.

The second part of this question, the criteria used to identify meteorological regions for analyses and how should specific monitoring data be selected from within each region, I think we have agreed that the weather service sites are the best. But one of the issues is that they are only in the major metropolitan areas.

What is the future of the ASOS? Is that a good backup to the weather service data?

DR. REISS: Yes, that's really the replacement to it. It came on-line -- I think the first stations came on-line in the early 90s. It really got going in the mid-90s. Now there are in some states more than a dozen stations, maybe more in California.

So it's a great data source. There are some limitations, as I mentioned, relating to the cloud cover. It is just not an easy variable to measure in an automated manner. But it is an incredible, rich data source that covers the country.

DR. MAXWELL: Then with California and Florida both being rather large states, alternate sources of data could be regulatory, state, local sites as well as industry areas. There may happen to be some industries monitoring data in the vicinity of where the PERFUM model may be applied. So that is just an idea.

Third part, comment on the manner that data from selected various stations were processed. We have gone over the stability class determination. Has any analysis been done on the differences between the Turner method which is like the Pasquell-Gifford method and the sigma theta and the DeltaT/ acceleradiation (ph).

DR. REISS: Yes. When DPR analyzed the CIMIS data, I think they looked at that issue and found that they were very comparable.

EPA has looked at that issue and found that these methods -- there are some differences, but they are all considered acceptable ways to calculate stability classes. And it didn't appear -- we looked at the distribution of stability classes across the station, and there didn't appear to be a bias.

DR. MAXWELL: We discussed the fourth one, does the panel agree with the approaches used to characterize those factors. We have gone over that.

It just seems that the FAWN data from Florida just doesn't seem to be worth much. I would kind of disqualify that type of data. You even mentioned you had to put in a lot of missing data or just leave it. That's not really that reliable.

Maybe the people in Florida will come up with something better like the folks in California have. Maybe you can talk to them and help them out.

Next subquestion there, what are the potential impacts of using data collected with different anemometer heights and analyses of this nature. That has been discussed also. Definitely, the wind is a lot more variable at the lower heights.

Has any analysis been done with using the power log equation on these different levels?

DR. REISS: Yes. As I said a moment ago, I think that equation fails for this small -- it is really meant for extrapolating above 10 meters, particularly in the nighttime stable conditions. It just predicts too big a difference between 2 and 10 meters. I don't think it is a valid way to adjust the data.

As I said, we'll try to put this data out there in the literature and it could help to refine that.

DR. MAXWELL: That would be one suggestion. It seems like you have covered a tremendous amount in your presentation and all the research you have done.

Some of the questions we're bringing up, it will be useful to maybe just address them. You have looked at this or that and here is what you have come up with, pretty much what you are explaining right now. And that would kind of alleviate a lot of the concerns that some other people may have had, that at least you have looked at a lot of different things.

I know it is tough to put everything on paper that you have done, but it might be a good idea to address those things.

How does PERFUM treat stability class inputs appropriately, we have gone over that. From what you have discussed yesterday and today, you mentioned there is not a whole lot of difference and they are comparable.

Then the last one, which I don't think we have really addressed on this that much, how does the PERFUM appropriately calculate boundary layer air concentration estimates by concurrently using upper bound meteorological and emission flux inputs? That's a loaded question there. That may be difficult to specifically address, but can you provide any more detail on that?

DR. REISS: Sure. The model, essentially by using five years of meteorological data, you are capturing the variability and you are including worst case situations in the data set.

Then by varying the flux probabilistically in the model, you are modeling in the correct proportions the probability that the worst case flux will occur with the worst case meteorological condition.

I think there are some issues like that Dr. Small raised about how we're treating that variability, which we might want to look into in a little more detail, particularly about the independence between the individual measurements that we get within the hours of the flux study.

But yes, I think the model is meant to treat both of those variables probabilistically. And that should account for at least the probability of a worst case situation.

DR. MAXWELL: Thank you. Just one question. This may be just to the EPA folks, but it was discussed that AERMOD is basically the current generation and the ISC3 model is the previous generation. Before that it was the old Crestar (ph) model in the 70s and 80s.

Is there any inkling when the AERMOD model may be basically blessed by EPA and considered an approved model?

MR. DAWSON: Unfortunately, we had an individual from Office of Air yesterday that could better answer that question, but we have looked into the same question ourselves.

I don't really have a good answer at this point. But certainly, it is something we're trying to keep our fingers on, that situation. And we consider going that direction when the thing finally comes out.

DR. MAXWELL: Thank you. That's all for my comments.

DR. ROBERTS: Dr. Spicer, when you are number five in line sometimes it is hard to come up with new things to say, but give it a shot.

DR. SPICER: I'll give it a go.

One of the comments that I would like to make regarding this idea of the estimate of the wind speed at 10 meters being different between D and F (ph) stabilities is that I got the opportunity to look at some data sets, some met data sets that were involved with the Kitt Fox carbon dioxide tests that were conducted at the national test site.

And the purpose of those was to look at how dense (ph) is an air (ph) gas, but also the idea was to try and do that under stable atmospheric conditions, which, of course, occurred shortly after sunset.

So what you could see in the data set was that that stable layer did develop, but it developed very slowly, and that the depth of it may only be a couple meters at a certain period of time.

I think that's the difference that you are seeing, is that those profiles are essentially steady state profiles. So they are making a 10 meter prediction on the basis of the developing boundary layer that develops for an infinite length of time.

DR. REISS: I think, ultimately, with this 2 meter and 10 meter issue it strengthens what we have done.

You have a concern that you are looking at a ground level source. And you are using data up at 10 meters.

I think the fact we have looked at both monitoring heights essentially reduces some of the uncertainty associated with those issues.

DR. SPICER: If I understand what you have done, I believe that I agree with you completely in the sense that you have used the anemometer information at 2 meters, but yet used the 2 meter and 10 meter information to determine stability class. I believe that would be the proper way of treating it and it would be consistent. I think that's a valid point.

Another comment made earlier was that there is no statistical significance between the exclusion zone predicted for the different met stations. I don't disagree with that.

But if you start looking at the map of California and you go down from Merced to Fresno to Bakersfield to Ventura, then there is a systematic decrease in the distance that you observe in the buffer length.

So it may indeed be a situation where someone might choose a value, that if you are given that opportunity there is some motivation for saying you need to choose met conditions that are close.

DR. REISS: I agree. If I said it, I didn't mean to say they were statistically significant.

I didn't really analyze. I think it was too small, the data set, to analyze it statistically. But you are right. There are actual variabilities. One of the more predictable variabilities is probably wind speed between these various regions.

But the standard deviation of the wind direction matters a whole lot. There is a lot of micrometeorological factors that influence that. So I think when you start to try to generalize among regions, that's where you get into a little trouble in making that generalization, is because of those micrometeorological factors that are affecting the standard deviation of wind direction.

DR. SPICER: That's exactly the point. There are micrometeorological factors that are extremely important. Therefore, local met conditions would trump, even seems to me, a very good data set at a remote location. So that's a real issue.

Then the question is not just associated with a model but then also associated with the guidance that's attached to that model.

It may be a situation where ultimately the regulatory agencies might want to consider that local met conditions could be monitored for a certain period of time and the use of those be accepted as opposed to using some remote location.

That would -- I don't know. That may not be workable from a regulatory point of view, but it is certainly something to consider. But if you open that can of worms, then the next can of worms is what sort of minimum data set would you need.

That might be something that I don't know whether you have considered with your five year set, can I choose, for example, a month out of one of the years and reproduce months in the other years.

DR. REISS: Yeah, we can certainly look at that with the model by just comparing the stability of the estimates across months and across years. We went with the five years because that is sort of the EPA historical recommendation.

DR. SPICER: Sure, and I can understand that. But if you are wanting to validate this idea of how much data do I actually need in order to use this methodology in some place else, then that would be a valid question that one could answer.

Obviously, comparing May to January, for example, would be a poor comparison. But May to May for specific years may be enough. It may be that a two month average are what you need and that sort of thing. It seems to me a logical thing to consider.

It also may be reasonable in terms of the consideration of this FAWN data set question. I think that your points are generally well taken that right now it seems that the FAWN data set is shaky. But since it is shaky, maybe there is a subset of it that can be actually used.

And so your five year information that's better from California, for example, may be able to inform you as to what minimum data set could be used in the FAWN to actually give a reliable picture of what is going on.

DR. REISS: With the FAWN, there are maybe six years available total for most of the stations. So when you look at a five year data set and you have a lot of problems, then that's telling you there may not be enough historical data to overcome that. It just hasn't been around that long.

DR. SPICER: There is no question that these sorts of things will continue to evolve. After all, several years ago one would have to actually go down and retrieve the records from the local airport in order to get this sort of data and it was handwritten. These things change.

I guess indeed the last question to me is the critical question. Does the model appropriately calculate the bounding air concentration estimates by concurrently using upper bound met and emission flux inputs.

I respect your answer, what you have done. Estimation of the flux is concerned -- is valid and then the use of the met data and considering the statistical uncertainties associated with those.

However, I think there is a lingering question associated with the atmospheric dispersion aspect of that. What you are assuming is that once you have a set of met conditions and once you have a flux, that when you apply the dispersion model that you are going to get a concentration at that distance that is that value.

And that's the typical problem associated with atmospheric dispersion. Even if you know the flux in a test condition, for example, and you know the atmospheric conditions, then the predictions may still only be within a factor of two. Granted, quite often they are better than that. But in a predictive mode -- and part of that has to do with the uncertainties in both the flux and the atmospheric stability conditions.

But I guess the point is that the dispersion coefficients do have significant uncertainties associated with them.

DR. REISS: Yes, and I think Dr. Hanna's recommendation about treating that probabilistically is an excellent one.

I would say that the estimates in this case are better than a factor of two just because of the nature of an area source and looking so close -- looking at concentrations so close to it.

With respect to the dispersion coefficients, ISC, at least in the regulatory mode, you can't run the model and vary the dispersion coefficients. You would have to actually go into the code and change that, which we can now do with PERFUM.

So it is not something I had considered before as a possibility. But how it is currently structured we could certainly treat that as a stochastic variable and get at more of the variabilities associated with the dispersion.

DR. SPICER: Certainly.

DR. ROBERTS: Thank you for your comments. Let me now open it to other members of the panel. Dr. Ou?

DR. OU: This is Li-Tse Ou, University of Florida. Since I'm from Florida, I have used the FEM (ph). But my main use of the FEM is the soil temperature.

We have our own temperature probe. We have quite a bit on temperature probe which have been calibrated at 24 hours before (ph) use. And we checked also temperature data with our local FEM station. We found that they are fairly consistent.

Unfortunately, my research did not involve wind speed and wind direction. I cannot have a comment about wind speed and wind direction. But the two stations I use is one in Gainesville and one 20 miles south of Gainesville.

DR. ROBERTS: Thank you. Dr. Wang?

DR. WANG: I'll just try to elaborate on the different data sources that you are using. I think it is a very possible approach to utilize all the different sources. But it seems the question is how to bring out these sources of data to a more common standard so that you can pick and choose without have to worry about where they come from.

One possible approach is probably create some kind of a calibration standard so that you can compare between these different sources of data from different pools.

I mentioned yesterday that there is another source called MERFLUX (ph). It is not sparse, but they use censors like radiometers or anemometers and then they use that as calibration standards within different stations so that you -- if there is a systematic bias in one set of the network, you may be able to detect that and then make corrections later on.

Also other potential sources of data -- I wonder if any of the weather, (inaudible) and remote sensing, those kinds of things may be used to fill some of the gaps in places that you may think about.

DR. REISS: I'm really not familiar with how well these satellite measurements can characterize the surface boundary layer. It is not something I have looked at.

DR. WANG: Wind speed and some of the factors may -- I think is possible to tap into those. NOAA may have some more information on that.

Another point on the anemometer heights, micrometeorology has been brought up a couple times, is that -- you mentioned the parallel earlier, but at the very lower boundary layers, say to about 10 meters, you probably follow what's called log wind profile.

In most of these fumigated fields, they are pretty flat. There are two factors we tend to consider. One, we call displacement height. In this case it is probably about zero since there is no crops, no trees.

The other thing is roughness length, which has to do with the surface conditions. If it is tarped, probably worse, mostly. If it is bare soil, then that's a little bit different. If you have bedded fields that may treat a little bit, you know, some more roughness. It depends on the wind direction.

So if you have two heights of measurements, you may be able to standardize your heights to one, say, two meter or one-half and use that among different locations to help you to possibly to homogenize your predictions at different locales.

DR. REISS: The roughness length is something I'm interested in. It is a variable in the AERMOD model, which is required. So it is something -- if we go to

AERMOD, it is something that we can take into account and would affect the turbulence. It is not something you can account for in ISC explicitly.

DR. ROBERTS: Dr. Seiber and then Dr. Baker.

DR. SEIBER: I want to, again, get some clarification, I suppose, on the strategy for selecting meteorology data. And I understand that PERFUM is a tool that will be used to essentially develop a strategy nationally and regionally for setting buffer zones. That's my understanding.

But it also seems that PERFUM would or could be used to help make decisions at kind of a local level. When a decision needs to be made on treatment of a field or set of fields and they lie close or within the general vicinity of some sensitive area, a subdivision or whatever, that PERFUM would be used in that situation as well, not just as a look up on a chart, but actually the model could be used to help make decisions.

And if that's true, that's where I think some of us, and I pick it up from some of the other panel members, want to know more about meteorology that's close to a site that's kind of -- if not site specific, about as close as you can get, not only in terms of geography, but also in time.

So there might need to be a look at what is going on last week and the week before and predicted for the week of the potential application. So we just wanted to see if that kind of input has been considered as part of this.

DR. REISS: I haven't really considered it, but there is no reason why the model couldn't be used for that purpose. If you have a reliable data set of any length, you can use that in PERFUM to calculate concentrations and margins of exposure and buffer lengths.

It is going to be an issue of commercial and feasibility and regulatory acceptance or maybe it would just be for research purposes. But that's really a policy decision I couldn't answer.

But there is no reason the model can't be used for that purpose if you had a data set that you could reliably say was reflective of that certain situation you have.

DR. ROBERTS: Dr. Baker?

DR. BAKER: As an employee of a company that sponsored the Kitt Fox project, I'm glad to see that it is being used.

The Kitt Fox, from a meteorological point of view, looked at the low wind speed of stability. And several of the questions that are raised here are generic to the ISC, its formulation and how well it has been calibrated against field studies.

So the more field studies, whether it is ISC or AERMOD, the more confidence we could have in the models. That was my main point. Thanks.

DR. ROBERTS: Dr. Yates?

DR. YATES: Just to follow up a little bit on Dr. Seiber's comment. It seems like it would be kind of nice if it would be possible in a place where you wanted to have some information in a lower area, say, like there was a subdivision which -- you know, if you think about California, things are growing there pretty fast, the thing that I could see being a problem though would be having a long-term record of meteorological conditions in that area.

And I was wondering if you thought it might be possible to take not a long-term record but say six months or maybe a year worth of meteorological data and then try to correlate with long-term records that you would use from nearby met stations, say National Weather Service, and if you don't see any kind of bias, then you can use the long-term data to do your analysis?

DR. REISS: Yeah. That would be kind of a bridging study. I think that -- it may work or may not work for a particular site. Yes, you could check that out.

Also, Dr. Hanna's idea, there are these national data sets that are model predicted wind speeds and wind directions from like the MM5 model. It is possible that you could look at that as a potential source of data if you didn't have an actual measurement site if you wanted to look at a site specific situation.

It wouldn't account for any kind of micrometeorological variation, obviously. You would have to be sure that that wasn't a big factor.

DR. ROBERTS: Dr. Baker?

DR. BAKER: To that issue, I am aware of a study in the Los Angeles area, so it is urban air toxics. It is the Bates 2 (ph) where the MM5 field was used to develop pseudo ISC stations at a number of points within the air shed to test to see how well that would do versus other weather stations that provide ISC ready files for matching the Bates 2 data.

It was difficult to match the Bates 2 data using any of the approaches. So it is hard to say. But at least there is a protocol for extracting MM5 information in developing sort of pseudo ISC station information.

DR. REISS: That's good to know. Thanks.

DR. ROBERTS: Is there anything anyone else would like to add on question four, Dr. Portier?

DR. PORTIER: This is a question that kind of -- or a comment that brings question three and question four together. When you look back at the methodology that's used to run this model and build a model, you are attempting to look at two concepts, uncertainty and variability.

When we're looking at the meteorological data your attempt is to use the five years worth of data to bring variability into the model. And then you are assessing that variability over that period to look at the distribution of boundaries and crossover points. Right?

In the flux discussion, the issue was uncertainty, where we're not quite sure what the flux estimate should be, so we're going to put some bounds on it and let it vary around.

The problem I had with the way you ran this is you confounded the two in the runs. So on one day you would have a certain day's meteorological data and you changed the fluxes on that day.

Typically, when we have run probabilistic risk assessments, we put uncertainty on one side and variability on the other.

So you pick a set of flux values and you run the whole five years and you get one snapshot of what might happen if this were the true flux on this field, and this field was treated on any one of 1,825 days.
And then I would go back, change the flux set and run it

again. And I think you are going to have to think about this as you develop the tables that you are going to use if this PERFUM model is used as a management tool to establish boundaries.
You are going to need to do this true to the Monte Carlo rather than a one D kind of situation.

I don't know where this comment needs to go, but I think Dr. Reiss understands what is going -- Mr. Dawson understands that the uncertainty issue puts confidence bounds on the probabilities distributions that you get by running the five years worth of data.

DR. ROBERTS: Dr. Small I think wants to add to that.

DR. SMALL: I agree with that. That's a good insight there.

I would mention also, then, if you want some of the uncertainties that Dr. Hanna mentioned in the parameterization of the atmospheric transport model dispersion coefficients and the relationships, could also be sort of one time selected like the emission rate before running the five years of meteorology in order, again, to characterize uncertainty and keep it separate from variability. You could have those sampled in some way. But keep your variability and your uncertainty distributions separate in the way that Dr. Portier suggested.

DR. REISS: That's an interesting comment. It is probably something we should take a look at. It has some computational challenges, given how long it takes to do one run. Maybe we want to do a sensitivity test and see how different that result comes out.

But I certainly would be concerned about the computational challenges associated with that.

DR. PORTIER: Someone has to come up with things keeping supercomputers busy. This is an obvious situation. To do a true 2D Monte Carlo, it is going to take weeks on a PC.

But you only have to do it once once we get it figured out. We'll find some machine somewhere.

DR. ROBERTS: Dr. Baker?

DR. BAKER: There are a couple ongoing studies of urban air toxics sponsored by the EPA and through trade associations working in cooperation with EPA that are attempting to develop a protocol for varying many of the parameters, including the sigmas and other parameters that are usually hardwired into ISC.

I saw in your references you did have communications with Steve Hanna (ph) who is working with John Erwin (ph) at the EPA on protocols of this type.

So as those studies evolve and those protocols are tested out and evolve, that would be a good place. But to do it right now you would be braving areas, new areas that other people are already looking at, as well as the computational problems.

DR. REISS: That's right. I'm aware of what Dr. Steve Hanna has been doing in that area, like in Houston, I think.

But, yes, it is an interesting idea. It has also the drawback of running ISC in a nonregulatory fashion. But from a scientific standpoint it sounds very sound and is something we might want to pursue.

DR. ROBERTS: Any other thoughts on question four?

Let me ask the Agency then whether or not the panel's responses to this question are clear?

DR. METZGER: Mike Metzger, EPA. I would like to kind of restate or extrapolate from what I thought I heard the comments from the panel on the 2 meter versus the 10 meter.

DR. ROBERTS: Sure.

DR. METZGER: Could we conclude from the recommendation that it would be best to use National Weather Service data that generally speaking we would not significantly underestimate edge of field exposures using 10 meter data versus using 2 meter data, since our main goal is to be protective for people that would be at the edges of the fields?

DR. ROBERTS: Let me let the panel respond to that. Is that interpretation or comments correct? Dr. Hanna, since you are lead discussant, I'll put you on the spot.

DR. HANNA: I guess from our discussions, at least looking at the data presented in this study and looking at -- and considering the variability in this layer in general, the two meter data can be, if needed to be included, can be used.

And as Dr. Reiss mentioned, it will add to the band or the spectrum of the uncertainty or the variability that we expect to see or we will see within a kind of a modeling application.

I myself prefer to have a consistent source of data even if we are looking at the variability. I prefer that we have the National Weather Service data as the 10 meter data. Not only because of that, but because of the quality assurance and the quality control.

And that's, again, one of the factors that we will seriously look at if we're using the 10 meter or even the 2 meter data or even the 10 meter. What quality assurance or quality control data application were imposed on the data.

DR. ROBERTS: But I think part of your response was the 10 meter data is conservative. Is that what you had heard?

DR. HANNA: The 10 meter data is considered to be more representative of the surface conditions in general.

That is the standard, I think, the regulation of the National Weather Service, is they put their towers at 6 to 10 meter, but mainly at the 10 meter height. That is the regulation.

Two meters can come from different kinds of observation systems.

DR. ROBERTS: Dr. Bartlett?

DR. BARTLETT: I think there is a concern in that your question as far as whether you might have an underestimation bias. I think what we have talked about before in a lot of micrometeorological conditions it would underestimate the buffer zone.

You might have more stable air and lower wind speeds. And so I think that to me -- I understand that as far as comparative purposes from different regions we're pretty much stuck with the 10 meter data.

But in some areas, the differences between 10 and 2 may be significant in terms of stability and wind speed. So we will be underestimating buffer zones.

DR. ROBERTS: Dr. Portier?

DR. PORTIER: Did you say the 10 meter is more stable or 2 meters?

DR. BARTLETT: I'm saying in some micrometeorological conditions, when we start talking about terrain and other real world generalizations, I'm saying we would be underestimating buffer zones by using 10 meter data.

Because you can have more stable conditions closer to the ground in certain times of the day and the wind speeds can be lower.

I would like other members to correct me on that if I'm wrong. That's my feeling or my belief.

DR. ROBERTS: Dr. Spicer and then Dr. Yates.

DR. SPICER: Dr. Bartlett's interpretation of the situation I believe is correct and consistent with what was observed in Kitt Fox, that 10 meter wind speeds were higher and you could have a developing stable layer near the ground, which for an area source would be significant as opposed to an elevated source.

DR. ROBERTS: Dr. Yates?

DR. YATES: There are conditions that are possible on a field, for example, if a field would be irrigated, which is one of the strategies for trying to reduce emissions, if you irrigate a field in a dry climate you can get cooling at the surface which could create a stable atmosphere above the soil.

And yet if you are using met data from somewhere else it may not be representative at all. So I agree I think that the local conditions can really have a dramatic effect.

DR. ROBERTS: Dr. Baker.

DR. BAKER: I believe what we're saying meteorologically is correct. I just can't process the data quick enough to know whether or not that's going to be a significant impact at the 95th percent of confidence.

DR. REISS: From the estimates we have, it is not a significant difference.

DR. ROBERTS: Are there other aspects that would be helpful for us to clarify in terms of our responses? Dr. Bartlett?

DR. BARTLETT: In response to your study on the 95 percentile, the phenomenon we're talking about is not for the flat study areas that you have described.

We're talking about, I believe, different geographic conditions that may be fairly common in certain areas of application.

DR. ROBERTS: Dr. Spicer.

DR. SPICER: I agree with that completely. But also the Kitt Fox tests were conducted in Frenchman Flat, which, of course, is a dry, light bed, perfectly flat.

You can even see the developing boundary layer there that would be totally missed by the data set that you are looking at because of even the time averages that are involved and also the elevations of the instrumentation.

There are effects that would literally go under what you are looking at.

DR. ROBERTS: Mr. Dawson?

MR. DAWSON: I was going to suggest it sounds like ultimately when we implement this model we're going to have to have some sort of selection criteria or something of that nature in place that accounts for these different parameters.

We're going to have to rank them in some way and consider, for example, distances versus data quality versus sampling height, those kind of things. All these are very good and helpful factors for us to carry back in that kind of process.

I did have one additional comment, or actually it is a request, that there were several specific sources of information mentioned. For example, there was a network that sounded like in the south Mid-Atlantic region called CRONOS and the MM5.

So any kind of specifics that you could provide in the report about those, that would be great.

DR. ROBERTS: We'll try and put some information in the minutes that helps the Agency access those sources of information.

Anything else that you would like us to clarify on this particular topic?

Okay. It's 10 o'clock, let's go ahead and take a 10 minute break. Then we'll come back and tackle question five.

(Thereupon, a brief break was taken.)

DR. ROBERTS: Could you go ahead and pose question five to the panel, please.

MR. DAWSON: The Agency model, ISCST3, is the basis for the PERFUM approach. This model has been peer reviewed and is commonly used for regulatory purposes by the Agency.

PERFUM also uses other Agency systems such as PCRAMMET. Please recommend any parameters that should be altered to optimize the manner that they are used in PERFUM.

Does the panel agree with the manner in which the receptor grid was developed. And if not, please provide suggestions for improving this approach. ISCST3, as integrated into PERFUM, was run assuming rural flat terrain which would be typical of treated farm fields but might not be typical of surrounding residential areas.

Does the panel concur with this approach? What are the implications of such an approach? What improvements can be made to this approach? ISCST3, as integrated into PERFUM, was run in a regulatory mode which includes the use of the calms processing routine.

Does the panel concur with this approach? If not, please suggest a suitable alternative.

DR. ROBERTS: Dr. Baker, could you please lead off the discussion in response to this question?

DR. BAKER: I believe ISC is qualified, well suited for the type of modeling. It is fairly standard.

It is certainly a step up from running ISC almost in a screening type of mode, which was the one meteorologic condition of wind combination, wind speed and stability and it allows for the probabilistic analysis.

I wouldn't move to a regional model. Calpuff was mentioned. I don't think I would start with any grid based model because I would miss the resolution. So I believe ISC is the choice I would have made as well.

For a rural region, flat, we have a nonbuoyant passive emission source. I believe the rural condition is the appropriate condition. I think running it -- my experience running this type of source in an urban mode increases the surface roughness and actually gives you lower concentrations downwind. So I think this is conservative.

In terms of the gridding, we did have some prior discussion on the grid concerning the computational, possible computational efficiencies that could be looked at.

And also you informed us of the alternate approach of gridding that you looked at instead of using the spokes, just using I believe it was a rectangular grid approach more recently. Is that correct?

DR. REISS: Yes. That approach would also have spokes and rings just like the other approach. It would just define them in terms of the rectangle instead of a square.

DR. BAKER: I believe the flat terrain is certainly the easiest to work with and to demonstrate the use of the model. It is the easiest to generalize. I agree, there are locations that may not necessarily fall into the category of flat terrain.

I just have one previous experience working in complex terrain that was with a dense gas model. And in that case, we were fairly satisfied from some field data and from discussion around that and our modeling, that the dense gas in the concentrations we were interested in was just actually following the terrain and actually could be simulated as -- even though the terrain was complex, running it as a one dimensional flat terrain model gave us reasonable results.

From a modeling perspective handling the calms, the calms processing is part of the methodology, part of the protocol for running the ISC. It is one of the assumptions built-in as a -- it has many assumptions.

The better these assumptions are captured in field data and the field data is used to calibrate -- was used in the calibration of, say, the ISC model, the higher the confidence can be. I don't know of any other alternate way of handling the processing of the calms.

So to the extent that that is captured in the field data for which the ISC model is calibrated against, I think we were satisfied in that respect.

That's all I have.

DR. ROBERTS: Thank you, Dr. Baker. Dr. Hanna, your thoughts on this one?

DR. HANNA: I agree with Dr. Baker's assessment in addressing these questions.

For the application on the kind of non rural areas or residential areas, I think that -- I mean other meteorological conditions can be used for assessing the model performance, the ISCST3 model.

For the terrain effects of complex terrain effects, it might be better really to use the AERMOD, which I think Dr. Reiss said that is the direction you are going to go through. And the AERMOD really treats the terrain in a better, more realistic formulation than the ISCST3 model.

The calm wind still again is essentially the mathematical way of getting around the zero wind speed and the ISCST3.

I agree with Dr. Baker also, that the best way for the validation of this model is the ISCST3 model or the AERMOD model against field experiment and see what kind of biases or how the model is performing over a certain case studies or periods of formulations.

DR. ROBERTS: Thank you, Dr. Hanna.

Dr. Ou?

DR. OU: I think the most important factor in respect to a separate (ph) buffer zone is the emission. I know you carried out a six theories (ph) on the six locations. But you only did it once. You don't carry out two times at a different season for each location.

Since I'm from Florida, I'll give you an example I found on (inaudible) location. You carried out the experiment in January in (inaudible). And the temperature between the wintertime and the summertime could be substantial.

And I'll give you my experience. I found one commercial fumigant near or against the area. They fluctuate between -- I did it once in winter and once in summer at the same site and used the same soil type, not in this location, but a nearby location with the same soil type. It fluctuates a substantial difference. Summertime, the first time could be three to four times greater than the wintertime.

And my question to you say you separate (ph) the flux rate at the Plant City site, say, on a 10 microgram, just assume a 10 microgram per square meter per second, do you use this flux rate to simulate the all year round or do you change the flux rate?

DR. REISS: Well, we have conducted the field studies in a variety of seasons. When we apply the PERFUM model, we just take the flux rate from a single study and apply it to the whole year and then do the same for every other study.

DR. OU: But when you assume at the Plant City site -- assume the 10 microgram per square meter per segment used this rate, assuming the whole year round for this particular site. Right?

DR. REISS: That's correct. I agree that there is uncertainty associated with extrapolating between different seasons. These studies are very expensive and it takes a lot of effort to get one data point and we are continuing to --

DR. OU: Somebody has to do it. Otherwise, it could be a few factor difference.

DR. REISS: It won't be a few factor difference, however. Because we're concerned about the emissions over the first 24 hours. At the Plant City site I believe the flux rate or the amount of the emissions over the first 24 hours was 57 percent of the application, if I remember correctly.

DR. OU: You did not carry out experiment to prove the difference is small for the first 24 hours. They are different now, cold season and hot season.

DR. REISS: I understand. There could certainly be a difference between the cold season and the hot season. I'm just making the point that because what we found in the cold season was 55 or 57 percent of the material emitting during the first day of application, that bounds what the potential error could be.

It couldn't be more than a factor of 2 for sure because there is just not enough mass in the system to do that. I agree it is desirable we get more data from more seasons and we're continuing to collect these data.

They are very hard to get and it takes a lot of effort to get. And it could turn out -- I think as Dr. Yates, we were discussing yesterday, that for this particular compound, the chemical, physicochemical properties, it could be a diffusion limited phenomenon that's not as highly dependent on temperature as other compounds.

DR. OU: Because you mentioned the buffer zone in the wintertime and summertime. Since you did not account for the flux rate, maybe inflate the summertime, buffer zone for summertime may be larger (ph) than the wintertime. Do you see what I mean? Because of the difference in the flux rate.

DR. REISS: I agree it is an assumption we're making. It is an uncertainty in the analysis, sure.

DR. OU: The other thing, since I'm from Florida, and during the summertime there is the possibility of 50 percent of the thundershower in Florida, and it usually occurs in the afternoon.

If you apply methyl iodide in the morning, and as thundershower occurs in the afternoon, since the methyl iodide is quite fairly water soluble, thundershowers bring most of the methyl iodide down from the surface atmosphere to the ground.

DR. REISS: We actually observed that at the Plant City site, where it rained not on the first day but the day after and the third day after the application.

And you are right, it washed the iodomethane out of the atmosphere.

I didn't make any assumptions in the model about rain, because we're only interested in the first 24 hours. We just assumed that people wouldn't apply during a forecast for heavy rain.

DR. OU: I mentioned it since your approach, your software approach is the probabilistic approach. Maybe you could account for the thundershower in certain regions.

DR. REISS: It is certainly a possibility.

You have to consider whether somebody is aware of the forecast that it is going to rain and for that reason doesn't apply. For that reason, we didn't try to incorporate it.

But particularly if we choose to look at the profile after 24 hours, that's something we might want to take a look at. Because we have some data as to what happens during the rain storm.

DR. OU: The other note, comment I have is I noticed somewhere near by the field they may have a small forest or nearby there may be a tall crop such as corn which may be two to three feet tall, and, of course, trees are much taller. I don't know how much effect on plume when the plume go the area.

DR. REISS: I think we have made the conservative assumption assuming flat terrain. If there was a cornfield downwind, then that would increase the roughness and turbulence and would likely increase the dispersion.

But because we're not developing this for a site specific scenario, we're trying to develop it for a general scenario, I think the appropriate thing was to assume flat terrain.

Now, if somebody wanted to apply the model for a particular circumstance, a particular field, then you would be justified in including the terrain in the calculations. DR. ROBERTS: Dr. Wang and then Dr. Seiber.

DR. WANG: I would like to comment on three of the four questions raised by the EPA here.

The first comment I would like to touch on is the parameter optimization. It appears the main inputs in the model would be field size, atmospheric conditions, application techniques, and the field emissions associated with those application, different application methods.

I want to touch on the application methods and their associated emissions. ISC model was not really written to treat these variations in terms of fumigation techniques, since it is written for different purposes.

But in this case you do have broadcast shank injection versus a drip. On the surface you may have tarp versus bare soil. Even for the chemigation you may have drip versus possibly sprinkler watering from above.

I think it would be more advantageous somehow to incorporate these different techniques and their associated differences in terms of contributing to the emission fluxes in PERFUM.

DR. REISS: The model does take it into account, the application method into account, in the sense that it uses the measured flux rate from the field study specific to the application method.

And that's largely how methylbromide is handled in California. I mean, the model can't do any anything else other than assume a different flux rate that's appropriate for that particular application method.

Now, when you move to AERMOD, there are some more things you could do in terms of roughness length of a raised bed versus a flat fume or broadcast application that might define things a little more.

But we do take into account the application method in the sense that we are using flux rates specific to a given application method.

DR. WANG: So it is using, basically, a lumped effect looking at the flux as a function of time and then treating that as an input to look at the dispersion processes?

DR. REISS: That's right. For each field study we have done specific to an application rate, we have a profile of the flux versus time for that application method in that site and the model explicitly treats that.

DR. WANG: But it is also a function of the application techniques. But although your results, comparing those three scenarios, I may say, show the variation that they may have a similar mean, if you compare the mean, they may turn out to be similar, since -- especially the drip and the two raised bed scenarios seem to have very similar outcomes.

DR. REISS: Yes.

DR. WANG: But, just in the general sense, these different techniques of application will in some cases, from experiments we have done in the past using direct measurements either aerodynamic or flux chamber techniques, they do probably show some systematic differences.

DR. REISS: It is quite possible. We talked a lot over the course of the last day about the factors that might affect the flux rate, including soil temperatures, soil -- you know, organic matter content, ambient temperature, application method, tarp thickness, whether the tarp -- what happens during the application. There is a lot of factors that potentially affect that variability.

If we can explicitly treat those, then we would. But at the moment the only thing we can differentiate is between the different application methods, and we need to try to treat the variability within that framework.

DR. WANG: Again, that leads back to the possibility of using some more mechanistic emission models that likely will incorporate those variables into that simulation so you will likely can differentiate those different methods. That's a long shot at the moment.

DR. REISS: I agree. If we can do that it would be great. But until that could be developed in a way that would meet regulatory standards and predicted the field data we had, we would have to -- I think we're better off using this more empirical approach.

DR. WANG: I would like to get on the second point, which is on the receptor grid.

It appears that the 120 -- well, you can convert to the milligrams per liter, which is the same as a microgram per cubic meter. Isn't it? Anyway, it is 120 milligram per liter concentration as a threshold, as a reference for developing the buffer zones. And that's one of the requirements you use to grid to delineate that region.

We recently finished this study. We did some literature search and it is not specifically on methyl iodide since there is nothing there, but some other related fumigants looked at their toxicity and exposure.

What we found was that the acute thermal LD50 of dazomet, dazomet was listed as two grams per kilogram in rabbits and rats, and the acute inhalation LD50 for the same chemical was 8.4 milligrams per liter in rats.

For humans the exposure for that, dazomet, was a low concentration that will cause skin, eye irritation, all that kind of stuff.

And the lethal oral dosage was 50 to 500 milligram per kilogram. And these have references that's actually reported by a USDA Forest Service study contracted through Information Ventures.

But I also have some data for chloropicrin on there and toxicity. Actually, this 120 milligram per liter was reported for chloropicrin as the lowest lethal concentration for cats, rabbits and guinea pigs.

That's 120 milligram per liter, if these animals are exposed to chloropicrin for 20 minutes it will cause death. I wonder if this 120 is also where you borrowed from or is it something else, some other unreported data just for methyl iodide. Can you elaborate on that?

DR. ROBERTS: Before you reply let me just interject. I think Mr. Dawson explained earlier that the 120 was simply inserted in for modeling purposes that the ultimate value that will be used is still under analysis by the Agency.

So they will presumably include and consider the studies such as you have mentioned and perhaps others to try and decide what the appropriate concentration would be.

For the purposes of our evaluation, we're not really commenting on that particular aspect, because it is a subject of a separate evaluation that the Agency has not yet completed.

If at the time they complete that analysis and want to bring that to the SAP for our comments, then we can comment on that.

But I would prefer that we confine our responses to the model itself rather than at particular concentration endpoint.

DR. REISS: I just want to make -- the registrant has developed an extensive toxicity database that EPA is reviewing to make that decision. I think you said the microgram per meter cubed is equivalent to a milligram per liter? I think they are a million fold.

DR. WANG: I guess it is milligram per cubic meter would be equivalent to microgram per liter, I think. It's the other way around.

DR. REISS: Microgram per meter cubed would be a million times, a milligram per liter.

DR. WANG: Microgram per liter would be equivalent to milligram per liter.

But anyway, if these study determine that these lethal concentration will change, then that will alter your boundary for the buffer zones. And that translates to your receptor grid definitions probably.

DR. REISS: Whatever the ultimate outcome of the toxicity evaluation will be incorporated into this risk assessment. And I should mention the 120 is not a -- never mind. You are right.

We'll incorporate that into the risk assessment.

DR. WANG: The last comments I would like to talk about is the usage of the assumption of the (inaudible) of flat terrain assumption that you used. I will say I agree with you. That is quite typical in most places where the fumigation is being conducted.

But in the case of nearby residential areas, I wonder if the micrometeorological conditions may be altered due to the presence of built environment, to houses, the structure itself and the trees that may be planted around it.

If those -- we talked about this earlier in previous question. If these will have an impact on the micro meteorological conditions, then maybe you need to take that into account somehow to help to be a more precise way determine the condition of the field sites.

DR. REISS: I think if you were looking at a site specific situation, then that would be a good idea. If you are trying to generalize to all, develop a national buffer zone, for example, then assuming flat terrain is probably the most conservative option you have and the only really feasible way you can look at that.

DR. ROBERTS: Thank you. Dr. Seiber?

DR. SEIBER: Just a brief comment. It has actually been brought up before. But since we were asked to comment on the receptor grid, again, it was developed in the documentation primarily for a square or regularly shaped field. And consideration should be made, maybe with some examples of grids suitable for irregularly shaped fields.

DR. REISS: We plan to do that.

DR. ROBERTS: Dr. Spicer, Dr. Yates, Dr. Bartlett.

DR. SPICER: With regard to this question about taking the obstacles into account with the dispersion model such as ISC, there are two effects I believe that are important with this.

The first of those effects I think is fairly well recognized, that is, that the obstacles that may be in the vicinity will tend to increase the surface roughness. So in other words, if you are next to a housing -- if you have a housing development in the area, then obviously the surface roughness will be increased.

The surface roughness is not something that's directly taken into account with ISC except in the rural urban coefficient question. But it is apparently included as a parameter in AERMOD, which is reasonable.

And so in the ISC context, then the housing development would have this urban flavor which would increase the dispersion coefficient and therefore decrease the concentrations and therefore decrease the buffer distances. So in that sense I think it is appropriate in ISC to use the rural coefficients.

The other effect, though, that you may get into with AERMOD, and I'm not familiar with AERMOD except just for some of the things that have been said, but obviously it does take into account the effect of surface roughness.

Surface roughness tends to have the effect on the dispersion models of doing things like increasing the friction velocity parameter. Therefore increases in surface roughness will result in increased dispersion rates.

But the other thing, the other effect you can have here, and I don't know if this is included in AERMOD or not, is the fact that when you have housing areas, for example, then in addition to increasing the surface roughness you also have physical obstructions to the flow which literally can slow the flow within the surface elements.

Now, for a ground level area source, that can become a significant problem, because of the fact that the material is near ground level. It can actually be moving in a speed that's lower than would be predicted by taking that sort of hold up into account.

And so the net result is that you can actually have concentrations that are higher within the surface roughness elements than would otherwise be predicted.

And so that's -- if you are looking at extending the methodology in PERFUM, then that's something to be considered.

And obviously this is not the same sort of toxic releases as occurred in Bhopal, but methyl isocyanate released in Bhopal, we believe that that's one of the things that was a significant factor. In analyzing the dispersion of that is the fact there was a housing area very close to the release and that the material actually got down in the housing area and was slowed down.

And there was a higher exposure to the people involved as a consequence of the fact that the wind speed was slowed by the housing.

That's something to consider in this. It is not as simplified an effect.

The other issue, of course, that has been discussed several times is this idea of calms. If I understand the ISC correctly, then the calms -- basically, you skip over that hour that's designated for a calm period.

And I believe that is not a conservative assumption as far as estimating the impact.

DR. REISS: Let me speak to that. The model is to a certain extent a calibrated model. And people have evaluated the model from a regulatory standpoint comparing it with tracer data. I think the statement that the calms processor or any other thing in ISC results in a lower concentration than you actually observe, I would be careful in making that statement.

This is a regulatory model that's been used by EPA for many years. And they built it in a way that it is appropriate for regulatory circumstances, which it would be inconceivable that there would be a bias, an overall bias toward a low prediction given its need to be used for regulatory circumstances.

DR. ROBERTS: Dr. Yates then Dr. Bartlett.

DR. YATES: I guess the thought I had from some of the previous discussion was that if you have -- if you are using PERFUMs for developing buffer zone information that could be used to kind of guide fumigations.

So this wouldn't be just doing any kind of a calibration with a field, but when you are starting to apply it in a regulatory way, if you had -- if you increase the roughness near the field, the context was with urban, like having houses and that, but say you are out in a flat area, rural, but you have a location that has a lot of trees or bushes or hedges, something that would increase the roughness around the field, that should theoretically reduce the buffer zone. Right?

DR. REISS: Theoretically.

DR. YATES: I suspect PERFUM would not give you any kind of a reduced buffer zone for that situation?

DR. REISS: Right. Right now it is not a variable. It is considered an ISC. But in the AERMOD model that hopefully will replace ISC pretty soon, you can account for surface roughness length and will make some adjustments to the turbulence as a result of that.

DR. YATES: It would seem like in California -- I know buffer zones are a real issue with the farming community because of the lost fields and economic issues and that, if there would be some kind of guidance that could help a farmer who is willing to put in -- this would be expensive to put in some kind of windrow or something like that, but if they plan to be there for many years and they look at the expense of something that is not too much, it might be a way that reduced buffer zones for fields that have these kind of windbreaks -- might be allowed given that there is some way to look at the risk of or the risk reduction by doing something like that. Just a comment.

DR. REISS: It is always possible. I would have to study that a lot further to see what they would need to do to make a meaningful difference.

DR. ROBERTS: Dr. Bartlett?

DR. BARTLETT: My question or actually comment concerns PCRAMMET and overlaps back to question four as a clarification.

The question about, basically, stability within the first 10 meters. I apologize for going back. But I think the clarification is necessary in thinking about what my colleagues have said about here, is that my -- the way I represented it before was in the context of the problem of different types of terrain conditions.

But actually, the question arises, even in flat terrain, is that to go from another location, apply stability conditions from a remote weather station to the first ten meters is probably not going to work.

And that having the wind speeds from one and a half and 10 meters may not be high enough resolution. And actually, as far as stability factors go, we probably need a high resolution stability measurements in order to actually -- for the dispersion model to work or in the sense of getting a buffer zone that would be accurate.

Actually, I think the discussion, the comment on Kitt Fox and that was flat terrain and did find that situation there. So that's I believe that's -- I'll leave it at that.

DR. REISS: I agree that using a remote weather station to look at an individual field, I mean, there is problems with doing that. You could have differences in micrometeorology that could affect things.

But I mean, what choice do we have in this situation where we're looking at products that are potentially applied to thousands of fields out of practice? So the goal is not going to be to try to accurately model each and every field. It is just not practical to do that.

What we really want to do is capture the variability that is potentially out there and ultimately setting that buffer zone at a level we're comfortable with assuming that variability is going to be safe.

DR. BARTLETT: I realize this is difficult, but it does, I think, reinforce the possibility that there is an underestimate of the buffer zone in the sense that you don't have monitoring stations, as far as I understand, outside the perimeter of the field. So you don't really have a validation for the dispersion beyond that first ring.

So it is hard for us to know the accuracy in between there. I realize we don't have the stability conditions for that, I believe the stability conditions are fine for ISC for close by, a relatively close by station for the upper levels of the atmosphere and for longer distance transport, for other situations.

But in such a short distance -- I'm just raising that as a question, there is probably a possibility of underestimation.

DR. REISS: I'm not sure I agree. I don't know why the bias would be toward under or overestimation in that case. We're not talking about long range transport. We're talking about a plume traveling just a few minutes to get to the threshold concentration.

I would mention we have a study currently in design where we will measure concentrations at a longer distance from the field in the predominant wind direction.

So we'll be able to take a look at that issue in a little more detail.

DR. ROBERTS: Dr. Baker then Dr. Portier.

DR. BAKER: You mentioned that with the field studies that were met stations, could you address the resolution of the time resolution for the wind speed and direction?

DR. REISS: Mostly minute data. I believe even out of the data loggers you could get up to five second data in some cases. We used hourly data because that's what is appropriate to use in the model.

DR. ROBERTS: Dr. Portier.

DR. PORTIER: As I listen to this discussion, I keep coming back to the idea that the coarseness of the data that's generated here, when you think about the concentration data that they are capturing in their grid around the field, I mean, they are capturing it as an integration of one to three hours, right, in the charcoal canisters.

You run that for three hours, then you send it off to a lab and it tells you what the concentration is. We're talking about meteorological data that seems to be on a much finer scale than that.

I worry that we are kind of beating them over the head with fine scale concepts when his measurement or their ability to measure is pretty crude at least on concentration stuff.

So I guess some of the more recent comments seem to imply we really need real time pictures of climate, but we don't have real time pictures of concentrations. We have chunk time pictures of concentrations. It may be that the data that we have, even the regional data, gives us enough of a chunk picture to be able to develop the kind of understandings.

But that's a question on my part.

DR. ROBERTS: I think Dr. Baker and Dr. Spicer would like to respond.

DR. BAKER: We have talked about separating out the flux uncertainties from the meteorological variability so fine scale meteorological information can be studied on its own for its own value. I wasn't specifically addressing the question of coupling the two. I agree that time scales aren't appropriate for that too.

DR. ROBERTS: Dr. Spicer?

DR. SPICER: I agree with you as far as that is concerned. I agree with Dr. Baker.

I think it is important when you are considering the flux measurements to look at maybe more accurate measurements even as far as the concentration is concerned if that's possible. And that falls in the same range associated with the vertical distribution as well.

I think that once you get around to implementing this as far as the regulations are concerned, though, I think that indeed you are going to find oneself in providing general guidance.

And all I was suggesting earlier by local measurements of meteorology is that when you find yourself in a critical situation where the general guidelines would indicate that you have some sort of difficulty, that it might be beneficial to make some sort of local measurements that might mitigate that situation in some way.

Or conversely, if you have a situation where drainage flows may be extremely important, then that would indicate that the general guidance would not be applicable and some sort of localized measurements would be appropriate.

DR. ROBERTS: Any other comments from panel members on this question?

Let me ask the Agency if there are any clarification or follow-up related questions on this topic?

MR. DAWSON: Actually, on this one I have several.

The first one was, somebody mentioned earlier on about the impact of thunderstorms and those issues. I want to make sure that -- I guess our plan at this point was to basically use the data as has been used in PERFUM and not try to incorporate, for example, the thunderstorm type of event or other significant weather events.

And I'm wondering about -- I guess is the panel comfortable with the conservative nature of that decision? Okay. Just for clarity, Dr. Wang had mentioned decoupling of the flux rates tied to application methods.

That is currently our approach. And I think it's reflective of the way that the DPR is doing it, and we're basically consistent with that, and we agree with that. That's our plan at this point, unless the data point us in another direction. Looking at all the chemicals we are looking at at this point, it doesn't seem to point to a different technique.

The other issue is on surface roughness and the question of conservativeness. So using it in a rural mode, that seems to be the conservative approach.

And is that sufficiently conservative to deal with those localized effects, for example, that Dr. Spicer was discussing or is there anything else we need to do over and above that?

I might also add that I was thinking about the ag drift model for aerosol spray drift. That one stuck in my head that the regulatory tier 1 approach in there is more of a flat terrain type of approach in there. So I guess what we're doing is in some ways analogous to that.

DR. ROBERTS: Let's take those one at a time. I think the first one had to do with thunderstorms and is it okay to sort of not consider those kind of weather events.

I'll point out and I qualify my comment that I have absolutely no meteorological expertise whatsoever, but I live in Florida. I can tell you there are parts of the year, especially between June and September, that there aren't very many days that you can state with high confidence that it's not going to rain.

With that personal observation, I'll let some of the experts to weigh in on that. I think Dr. Yates wanted to say something.

DR. YATES: A point of clarification. When you say not considering thunderstorms in that, you said that would be conservative. What do you mean by that, by conservative?

MR. DAWSON: I guess from the perspective of if we're developing like an assessment for large regions of the country or something of that nature. I think on a more localized level, we would certainly want to look at data that's more reminiscent of what is going on in particular fields or groups of fields.

It all depends upon how you implement and use this model. Starting, our first need is going to be to implement this on large regions of the country or on a national level, how ever you want to put it. So I guess I'm asking the question from that perspective.

DR. REISS: Can I jump in? Another reason why it is conservative is because the compound is soluble. We found that it rains, it washes it out of the atmosphere. So you are talking about much lower exposures when it rains.

DR. YATES: I was going to say that during a rain event you are right. It seals the pores of the soil which acts as a diffusion barrier. That's fine. But we also have been asked a lot about the rare events, and does this thing capture the rare events?

There is one way where thunderstorms can have a significant effect on emissions. There has been some research that's been done looking at the barometric pressure that goes with storms. And if you -- under certain conditions you could have large changes in barometric pressure that actually cause a convective flux of the chemical out of the soil.

This would be where the storm is nearby, not raining on the soil that's been fumigated. So you are talking about a rare thing now.

But you can have very large fluxes for a very short period of time. And if it happens to coincide with fumigation, then you definitely are not getting the conservative estimate. You are missing that rare event.

DR. ROBERTS: Dr. Portier.

DR. PORTIER: I made a note to myself and I'm glad you brought it back up again. Because one of the statements you said is that while they are unlikely to apply the material on a rainy day, at least a day where it is raining in the morning when you plan to go out and apply the material, and if this simulation is meant to simulate the 24 hour exposure on the day that it is applied, you probably should be looking at those five years worth of data and excluding those days that have rainfall in the morning. Because they are going to have stability differences, temperature differences that really differ from the kind of day that applications are going to occur.

I hate to do that because in Florida you are going to throw out everything in the summer for most of these sites.

On the other hand, if we start talking about the rainfall stations that are not adjacent to the sites you are looking at, you have to say, well, is rain at a weather station 200 miles away --

DR. REISS: It is complicating. You would have to bring in another full data set to the model.

DR. PORTIER: You would have to bring in hourly rainfall data.

DR. REISS: I'm not sure if it would be worth the benefit. It would be a potential refinement, but it would be a lot of effort, and I'm not sure --

DR. PORTIER: The whole point here is that is the conditions under which you are trying to run this model. And if rain is a big factor in changing the conditions, I think it is a factor that has to be taken into account, at least morning rain. Right?

DR. ROBERTS: Dr. Wang.

DR. WANG: I agree that considering the storm events would be a conservative approach if you only look at the 24 hour concentrations.

But in the last few years we have been running field experiments, fumigation experiments every year or every other year. Almost every experience, we're going to run into rain, not on the day of application, maybe not even be the next day or next two days, but it could occur in three days or four days. Sometimes it may occur the day after.

What it does is it prolongs emission flux. So they may not come out in 24 hours, but you have more emission, the emission may be delayed, you may say. Because some of the compounds, the hydrolysis may not be a main pathway for degradation, so they are kept in there. They come out eventually.

But from exposure standpoint, that could pose a risk. If a long term exposure becomes a concern, then how that may need to be considered in your risk assessment may come into play. Somehow you may want to consider that and add another twist to the model somehow.

MR. DAWSON: I think we're going to have to look carefully when we look at these as individual cases and look at the emission profiles and also the duration of exposure issue that we were talking about yesterday.

DR. ROBERTS: Dr. Winegar, I think, had a comment.

DR. WINEGAR: As a Californian who actually grew up in the Northeast, I tell my kids about how it actually rains in the summer sometimes in other parts of the country. The comments about the rain in Florida is kind of -- is the contrast in California where it rarely rains in the summertime.

So I think this idea argues against the use of a generalized specific or a generalized meteorological conditions across the board.

DR. ROBERTS: Dr. Baker.

DR. BAKER: In theory, I guess, but I'm still not convinced that rain would make that large of an underestimate of the buffer zone.

DR. REISS: I don't see how it would result in an underestimate in most circumstances, not withstanding Dr. Yates' concern. I have not heard of that data, but in most circumstances it should wash out the chemical and reduce the concentrations.

DR. ROBERTS: Dr. Portier?

DR. PORTIER: I was thinking about this. If you include a lot of rainy days and the effect of rain is to extend or prolong emissions, that's going to change your probability distributions. It is going to change -- you're going to have much more low emissions in the distribution, which could tend to pull the tail in a little bit.

DR. REISS: In this -- particular to this chemical, we're looking at the first 24 hours. Because in the studies we have done, it has been more than twofold higher than the following 24 hours.

If there is a circumstance where that could change, where you could get a peak later on, I would have to look at it. The data that Dr. Yates has pointed out, maybe we need to take a look at. But at this point I think we have been conservative in using just the 24 hour flux rates.

DR. ROBERTS: Anything else on the thunderstorm event? Was that feedback reasonably coherent?

MR. DAWSON: Yes. I think the bottom line for us is that we're going to have to -- because the various chemicals have different properties, we're going to have to look carefully at each of the cases and evaluate them.

DR. ROBERTS: The thought seemed to be that it would not cause an underestimation except for the aspect that Dr. Yates pointed out that probably bears some thought or examination because that's a situation where I can see that that might occur, but I have no idea.

I don't know that we can give you an opinion about how much that would affect the model.

What was number two on your list? Refresh my memory.

MR. DAWSON: Number two is really a clarification. I was talking about the decoupling, and basically that was our plan. Dr. Wang had commented on it earlier.

We were consistent with that approach and basically DPR is treating the emissions data for specific combinations of application methods as it decoupled. So we're looking at them individually. And that was our plan. So I guess it was consistent with what Dr. Wang was commenting on earlier.

DR. ROBERTS: The third was surface roughness.

MR. DAWSON: Right.

DR. ROBERTS: You want basically a clarification on whether the rural mode -- or a clearer feedback from us about whether using the rural mode is, in fact, conservative.

MR. DAWSON: Right, and what are the potential pitfalls associated with that, considering what Dr. Spicer had commented on earlier.

DR. ROBERTS: Let's get some feedback. Dr. Baker and then Dr. Spicer.

DR. BAKER: Within the constraints of the ISC model, which has been selected for this modeling exercise, you have the binary system of urban and rural within that constraint. The rural for this type of source would give a conservative, a large --the largest buffer zone versus the urban.

We did talk about different models that don't have the binary system, have a spectrum such as AERMOD, and in that case -- well, I guess when that time comes, the issue could be readdressed. But as currently configured, the modeling system is conservative in my opinion.

DR. ROBERTS: Dr. Spicer.

DR. SPICER: I agree with that completely. Since ISC is a binary choice, then the more conservative choice is to use the rural cases indeed.

And the only reason why I was bringing up the other issue is just simply because there was talk of AERMOD. And I do not know how AERMOD addresses that question. And I guess the other point is that there obviously are other dispersion models available at this point in time that do things like take into effect terrain effects and those sorts of things. But they are beyond the scope really of ISC.

DR. ROBERTS: Was that helpful?

MR. DAWSON: Yes. Thank you.

DR. ROBERTS: Let me ask then are there any other follow-up questions or any other aspects for which clarification would be useful regarding this particular question of the topic here?

MR. DAWSON: No, we're fine. Thank you.

DR. ROBERTS: Let's take the next question before lunch. I think since it doesn't have as many parts as some of the other ones it may not involve as much discussion.

MR. DAWSON: Question 6 is focussing on reporting of results. Soil fumigants can be used in different regions of the country under different conditions and they can be applied with a variety of equipment.

Please comment on whether the methodologies in PERFUM can be applied generically in order to assess a wide variety of fumigant uses. What considerations with regard to data needs and model inputs should be considered for such an effort?

DR. ROBERTS: Dr. Seiber, could you start out our discussion for this one?

DR. SEIBER: Yes, some of these things of course have been gone over. In fact, the discussion of rain in different parts of the country is very applicable. But I will go through what had occurred to me even though some of it might be repetitive. It seems to me to start off that PERFUM uses methodology, the back-calculation of flux, the ISC model, the MOE calculation as an add on. That's general methodology that could, in fact, be applicable with at most some modifications or adjustments to most growing regions in the United States, maybe all.

However, the difference is really in the calibration or validation runs that are needed to fit regions and sites. In other words, the applicability will need to be demonstrated that it in fact can be used in those other regions.

So I made kind of a listing of some variables that ought to be considered in looking at the region to region applicability. Again, some of these are redundant.

But first of all, temperature, air and soil can vary considerably from one part of the country to another and one region to another. And air temperatures can do a lot of things in your experimental design.

For example, air temperature combined with humidity can affect sampling efficiency through charcoal. That's something that needs to be considered when you run your calibration runs.

I think we have seen that in a few cases with methylbromide where we have to be careful that our analytical methods can respond to different temperature and moisture conditions.

Of course, air temperature and soil temperature, particularly soil temperature, can affect flux rates, can affect soil degradation rates and has, potentially -- with regard to soil degradation, there might be some effect on microbial degradation.

I know somebody earlier had brought up the potential for enhanced microbial adaptation and degradation when the fumigant is used more than once on the same piece of ground. And that might vary from one region to another.

It might be more pronounced in a soil that's rich in humus, for example. I don't really know that. I'm not a soil scientist. But it is something that I would want to take into account.

In addition to temperature, I would want to know something about the different water evaporation rates in different regions. I'm not sure we really hit on this too much, but some of the, for example, the CIMIS stations collect water evaporation rate. And that seems like it might be useful to examine as a potential correlant with fumigant flux rate.

I'll just stop for a second and ask whether water evaporation rate is data that's being collected or used in any way in your calibration runs?

DR. REISS: No. As you say, I believe it is available from CIMIS, but no, it is not something we have used in any calculations to date.

MR. DAWSON: We're talking about pan evaporation as the measurement?

DR. SEIBER: Right.

MR. DAWSON: As a basic, I guess, component of, I believe, the -- some of the environmental fate studies like field dissipation, I think that is one of the parameters that is collected. There is potentially some information we could mine.

DR. SEIBER: A third variable would be atmospheric moisture. Again, this gets back to the rain discussion. It's important in some parts of the country.

Rain really does two things. It can either moisten the surface and change the flux rate. In some cases it can essentially block it off if it is a tarped field and there is water that collects on top of the tarp. And moisture can also wash out. Rain can wash out downwind residue.

So in both cases, it mitigates, it seems to me, the downwind air concentrations. So these factors, I think, could be taken into account -- when you use this model, I think if it rains then you have kind of a subroutine that either might extend the residue, decrease the downwind concentration, do something that -- maybe could even be turned into a useful tool.

If there is any comments on these things if I missed something, let me know.

A fourth comment would be on physical obstructions or entities that exist around the field, and it has been commented upon.

There is something that exists in many parts of the country that don't exist in the San Joaquin Valley, and that's trees. They can be fairly pronounced. They can affect local wind movement. In fact, they are planted as windbreaks in many parts of the country for a variety of reasons.

They can also do another thing that I'm not sure we really alluded to yet in the discussion. They can serve as a deposition source. They can literally absorb residues. We looked at this for methylbromide. We didn't really see much effect of downwind vegetation as a sink for methylbromide. Whether that's the case for methyl iodide or maybe some of the other fumigants that this would be applied to, I think would need to be considered.

And that's not just trees. It probably ought to be checked out for other common crop canopies like corn or others.

And again, on the subject of obstructions or variations, obviously, hills, mountains and valleys are not too unusual around, near these growing regions. So they need to be taken into account as well.

And then from one region to another there could be important application variables. The use of tarping, use or non use, the type of tarping, the depth of injection and so forth could vary from one region to another.

And again, a variable that's quite different in some parts of the country is overhead irrigation issues. There are different types of irrigation. There is flood irrigation, overhead, drip, and then there is rainfall.

How water gets to the crop or water gets to the field would be important. One would assume they would turn off the overhead irrigation right after a fumigation, but that remains -- that should really be pursued and confirmed.

Then the final comment that I would have is from one region to another there could be differing levels of interest in air shed concentrations, not just the buffer zone.

And I know this kind of goes beyond the

discussion, but it has been brought up before. So I will just mention it again. It seems to me PERFUM could potentially be adapted to a larger spatial distance prediction. And I think Dr. Reiss mentioned that they are going to extend out the region of applicability, look farther downwind perhaps in the future.

And it can also I think with adaptation be used, although it is difficult when more than one field is applied simultaneously or in close sequence.

So again, that might be a concern in different parts of the country where you have air shed concerns.

I think I will just stop there.

DR. ROBERTS: Dr. Ou.

DR. OU: I totally agree with Dr. Seiber's comment. And I don't have much to say except to say that the PERFUM was developed based on the California DPR system for methylbromide.

As a result, PERFUM can predict the methylbromide buffer zone pretty good. But for the three others, (inaudible), vapor pressure and fumigant, once (inaudible) chloropicrin and MITC, I said they need to be validated, how they can be applied to the three, log (ph), vapor pressure and fumigant and provided by an independent validation. That's my comment.

DR. REISS: I would certainly agree some validation would be needed to apply to any kind of different chemicals.

DR. ROBERTS: Dr. Shokes?

DR. SHOKES: My comments may be related to some that already have been made. There will be some repetition. But I need to go ahead and make them anyway. That's why we are here.

There is, I think, some potential for generic use of PERFUM. It could probably be applied generically to evaluate other fumigants, with the considerations Dr. Ou just mentioned, in other regions.

But in present configuration, probably it seems to me it would be best for the highly volatile fumigants with that high initial emission from the soil. And certainly I would concur with the opinion that to use this model in other areas it would be essential to use regional or local weather data as close to the area of concern as possible.

I think one thing as weather networks are set up, most states are not fully aware of all the potential uses for this weather data. So we don't always collect all of the types of data that are needed and in making people aware of it.

I have certainly become aware of some things here that I wasn't aware of before, and we just recently set up another weather network here in Virginia. And so they are always asking what kind of weather data do you want to collect. And certainly there is tremendous potential out there to collect a lot of data.

The question is is somebody going to do something with it. Certainly you could use some things here that perhaps some aren't collecting. So making people aware of that could be helpful.

Data is frequently available through local weather networks as was mentioned by Dr. Hanna earlier with the CRONOS Network.

In Virginia if you were working in southeastern Virginia, we collect data on a Peanut/Cotton InfoNet that's used for different purposes. It is used for forecasting disease. It is used for predicting -- giving frost advisories.

It is used in the spring for letting people know when the soil temperature is right and the weather conditions are right to fumigate soil and only collected during the season, however.

And while we don't make the five year data or whatever accumulative data available over the internet, the current data is available and the cumulative data could be obtained by a simple e-mail and people do occasionally ask for that. And it is made available to them.

There were some uncertainties that affect the generic use of PERFUM that were mentioned and Dr. Seiber has already mentioned some of these, but on Page 90 the statement was made about the flux rate. We have had a lot of discussion of that and the various factors that could affect that such as temperature, organic matter, soil type, things like that.

When we talk about fumigants, as a plant pathologist, I look at the other end of it rather than what we're looking at here. I tend to look at what about the efficacy and how do we make it more efficacious. How do we make it work and how do we make it work at the absolute lowest rate possible.

I think that is a concern by the Agency and it would be a concern by everyone from a safety perspective that we make these things work at the lowest rates possible.

I think it was mentioned that some of these factors have not been quantified for fumigants and it would be difficult to do so. Maybe you could explain that a little bit. Soil physicists don't have too much trouble quantifying these things.

Is there not some way to look at those factors in relation to fumigants and what happens when they go in the soil?

DR. REISS: We have talked -- a number of panelists have mentioned mathematical models that are being developed to do that. There is a lot of factors for one. We have listed numerous factors. Right now we have really seven data points to work with.

And when you are developing a regulatory model, I think everybody would be reluctant to just apply a mathematical model that sort of accounted for all those factors without it being validated.

I think when a validated model is available it would increase the accuracy of PERFUM and any other kind of model looking at this.

But until that data, I think we're best relying on the empirical results we get from the field studies.

DR. SHOKES: I think it is probably possible to get some of that, though. And the more you could put into it, the less that uncertainty would be and the better idea we would have of how it could work.

Some of those factors could affect the rate of flux. I think that could be important. Another factor that was mentioned was the windrows or tree barriers. And that is a consideration in a lot of areas of the country.

Particularly, if you come to this part of the country, it is rare to find an agricultural field that's not surrounded by trees on two sides or possibly three sides. And the fields tend to be smaller and so those barriers are a major factor.

In our area we sometimes plant windrows because we have a problem with wind erosion because we have very sandy soils and spring winds that can move those soils.

So those are some things that could be taken into account. The rain was a factor. I know it was mentioned in the Florida area, rain in summertime was a major factor. In fact, I would interject at this point that part of the problem you had with the FAWN data was probably due to those frequent thunderstorms because those frequent thunderstorms tend to knock those weather stations off the air frequently.

And having done some disease forecasting work in Florida for many years, we did have a problem with that. It is one of the highest lighting strike areas in the United States.

That is a consideration there. So it is hard to -- it isn't that they don't want to keep them on there, it is just that sometimes that happens.

It is noted that the model adequately, I think, considers the atmospheric stability and computes the buffer zones. And if you could incorporate some of these other factors that might take a volatile fumigant such as methyl iodide and make it a little less volatile, it might be helpful -- or at least decrease the emission from the soil.

These parameters that we mentioned might affect that aspect, and they could be investigated and incorporated into a model that's been mentioned.

Also that sometimes people might want to take a model like this and use it predictively to actually make a recommendation, for example, as to when to fumigate. Obviously, if you have a tool like this, it could work quite well.

You would want to be able to do things like that and perhaps improve the response to that fumigant. If you could get the proper conditions for the safest application of that fumigant, then you could make that fumigation more effective, that application more effective.

In fact, that could work positively in looking at situations where you might even could decrease the rate of that fumigant, which would be a very positive thing.

One of the things that had occurred to me during this discussion in looking at the diurnal effects, I wondered has anyone ever looked at application if you had some at different times of day. Has anybody ever looked at nighttime application when the soil temperatures are cooler and would that have a significant effect on the fumigant?

DR. REISS: It probably would. I'm not aware of data. We at least don't have any for methyl iodide. You would get higher buffer zones if you were to apply it in the evening because you get that first burst of emissions during the more stable nighttime period.

DR. SHOKES: If you applied it during times when soils were cooler, would that burst of emission be as much as you think there?

DR. REISS: At this time I don't think I have a model that can predict the flux as a function of soil temperature. So I couldn't really answer, at least quantifiably answer that question.

DR. SHOKES: I was wondering if anybody had ever done that with methylbromide, checked it out, see if they can actually decrease the emission when the soils are cooler.

Because it seems to me the major problem with these highly volatile fumigants is that they are applied at very high rates because of the fact that you are losing, you are actually losing so much of it. In fact, your figures showed 35 to 61 percent of it within the first 24 hours. That's a significant loss.

If you could apply it -- in fact, it seems like the tarps are really not doing a whole lot of good in terms of containing the fumigant. And if you could -- and certainly applying fumigant and then tarping that soil during the daytime, that tarp causes that soil to heat up significantly after it is put down. So wouldn't it possibly be better to do it at night? I don't know.

DR. REISS: I don't think it would be good to do it at night despite the -- just the data we have with late applications, I mean, the ones that ended just before the early evening shows that there wasn't a discernible difference in the amount that came out.

Between the Oxnard study, which I think ended at 8 p.m. and the other raised bed study, there was about the same amount of the emissions that came off. Although those applications ended at very different times of day.

DR. SHOKES: What if you waited until early morning after that soil has had a chance to change temperature?

DR. REISS: Early morning is the ideal time I think to apply.

DR. SHOKES: I'm talking about 2 to 3 o'clock in the morning.

DR. REISS: It has not been tested as far as I'm aware.

DR. SHOKES: It might make a significance difference there. Such an application would be very possible and even practical today with the GPS and GIS equipment that we have where fields are mapped where you can do anything at night that you can do in the daytime.

DR. REISS: It is an interesting comment.

DR. SHOKES: It might be worth looking at, because if you could do that and you could get that emission down, you could lower those rates and the end result of that would be an improvement, not only possibly in the efficacy of the material but also in the environmental aspects of it with less load on the environment.

It is something worth looking at. Anyway, those are some things I just would like to consider there.

Another aspect of that is if you look -- as more data sets are developed, would it be possible to take a model such as this and develop different reference tables for soil types in a given region?

We have some really good weather data. We have not only five year weather data, we have 67 year weather data at our station. We can tell you pretty much what the weather has been over the last umpteen years.

But could it be possible to develop some scenarios to make it predictive? I always look at things from an extension perspective of how do you tell a producer that has to put out a fumigant what are the optimal conditions to put this out. And when would those likely occur. And we could use a model in that regard.

I always look at using it and turning it around the other way and use it to benefit, not just for the regulatory, but also - and that would help the regulatory issues if we're optimizing application.

DR. REISS: If you knew what the flux rate variability was with soil type or temperature, then you could certainly use the model for that purpose. My understanding, and I'm not an expert on these agricultural issues, but sometimes the growers have a pretty narrow window where they have to apply this product.

Also, it is quite a substantial contraption that's required to lay the shanks and lay the tarp and everything.

So they often have these commercial applicators in at a certain time. There are some issues about feasibility in terms of whether they can wait a week or a few days for an application to occur. But the model can certainly be a guide to answering some questions about what the potential benefits of that are.

DR. SHOKES: I would agree. You do have some pretty narrow windows sometimes. But if you are talking about regulatory issues of buffer zones and things like that, if optimization would improve that, you are willing to change that window a little bit to fit those things if you have to.

In that regard it could be a good thing to know those things. I'm just trying to look at ways that you can optimize application so as to decrease those buffer zones.

Because those buffer zones could preclude the agriculture and certain types of agriculture in some areas, particularly as I look at Florida and what you are showing there where urban encroachment is a significant problem in agricultural areas. In many parts of the east coast of the US, that is also true. That's all I have to say.

DR. ROBERTS: Thank you.

Dr. Yates, do you have any comments to add?

DR. YATES: Most of the things that I have have already been discussed. But I'll kind of go through it just for the record, I guess.

It seems that the methodology in PERFUM is fairly general and appears to be generally appropriate for methyl iodide as well as any of the other soil fumigants.

In fact, since there really isn't any pesticide specific information that you incorporate into it, it is just the flux rate, it seems like it would be appropriate for any volatile compound, really.

Given that, if you assume that there is no reaction once the chemical is in the atmosphere -- given any kind of a volatile chemical, you would get a conservative estimate. So it seems appropriate in a general sense to just be able to apply it to any pesticide or any volatile, I should say.

But it needs appropriate input information, flux rates, met data, which in many cases are available. I think that there needs to be some guidance to indicate situations where the model may break down or be inappropriate. We have discussed that in a number of the previous questions, geographical complex terrains.

Maybe a field -- like around here when I was flying in I noticed there were a lot of fields that were surrounded by basically forest.

I don't know what the effect of if you were to apply a fumigant in there what --whether the wind would actually be able to get down or if it just kind of goes over the top.

It could be that in a situation like that exposure might actually be quite different than what you would predict compared to something like California where it is quite open. So some kind of guidance or options for situations like that would be useful.

For the second question, what considerations with regard to data needs and model inputs should be considered, this is a real tough one. I think that what you would need depends a lot and what you are trying to do.

If you are looking at a very local like a field or a very localized region, the data requirements would be quite different. But I suspect that the real intent of this is to provide regulations that would be used statewide or at least over large regions.

So it would seem to me that the flux distribution with time should be something that represents the average over the state which might be difficult to obtain, but if you have it then at least it is the appropriate information, in my mind.

Then, there would have to be some kind of uncertainty or error information, information about errors that would also encompass that regional extent, state or large region.

The uncertainty I think should incorporate measurement air, modeling air and then translocation type effects such as moving from site to site, different soil types, different environmental conditions, timing of application -- the things we have talked about before. But given that you have that data, it seems to me that the model should provide some useful information on exposure.

On some of the previous discussion on this topic, a couple things were brought up that I just want to kind of say a couple things about. Fumigation is conducted -- this follows some of the comments of Dr. Shokes, fumigation really is an intended pathogen control.

This idea of making sure that we put the correct amount of chemical in the soil to control pathogens is an important one. Fumigants are often put in soil at much higher concentration than are needed because there is a lot of leakage through -- for example, for a flat fume, there would be a lot of leakage through a high density polyethylene film.

However, changing the film to something like a virtually impermeable film has the potential to reduce the escape of the chemical, which means that exposure time in the soil would be more. So in theory you should be able to use less chemical.

Actually, Dr. Wang did a study back about, I think, it was around '96 where we built some plots and we put virtually impermeable film called Hidebar (ph) on the soil. We actually dug trenches so that we could put this virtually impermeable film into the trench so we had no literal movement. It would be very similar to a large field experiment where the process occurs vertically.

He applied methylbromide at three rates. One was at the standard rate, at a 75 percent of standard and 50 percent of standard. We had a nematologist go in there and put some nematodes in at various steps.

We looked at the efficacy. It turned out that at 50 percent of the standard application rate, you still had some control. It wasn't perfect, but at 75 percent it seemed to me that there was no significant difference between that and the standard rate.

So if this could be applied -- this was done at a small scale. There were no seams in the tarp. So it was very idealized conditions. But if this could be applied at a field scale where you start having tarps put down and seams and all this, there is a potential to reduce emissions and maintain control.

And I guess in essence since flux drives the buffer zone size, if you reduce the flux, you reduce the buffer zone. It is kind of like everybody wins if something like that would work. This along with some other things has motivated us to also look at techniques to try to model pathogen control.

And this is some work that's fairly recent where with a soil based model it allows you to do things that you can't do with more atmospheric models.

But given that you can simulate the diffusion of the chemical in soil, if you have an exposure, what we call a mortality curve basically it relates mortality to exposure and time of a chemical, concentration time is what it's often called, you can in principle predict the zone of control.

So if you can couple something like that with the amount of fumigant used in principle, you could help the farmer to determine how much chemical they need to get the control they need, which in essence would help reduce emissions so they don't overapply.

If you have something at the surface that could keep emissions low, then you reduce emissions into the atmosphere. That would have the potential to reduce buffer zones and make it all work.

This model will -- to be able to do that in the full sense, you would have to be able to couple everything from the soil into the atmosphere. But if you had the experimental results for virtually impermeable films that show reduced emissions, PERFUMs could be used to determine the buffer zones appropriate for that kind of application technology.

That pretty much covers everything, I guess.

DR. ROBERTS: Thank you, Dr. Yates. Dr. Hanna, did you have some comments you want to add?

DR. HANNA: I just had some follow-ups to Dr. Seiber's comments about the application in different regions and different conditions. Of course, he mentioned the temperature and rain and other factors that we talked about during the meteorology question and discussion.

But another factor I think we need to mention is the conditions or regions with temperature inversion. That's the vertical profile of the temperature. That's very critical stability criteria of temperature profile in the vertical, not in the horizontal, only in the vertical where the temperature increases away from the surface going up because of radiation cooling of the surface during night hours in kind of land areas during summer.

This temperature inversion usually washes out during the hours of the morning, but I think this is one of the factors that should be accounted for when we are doing the modeling analysis. I don't know -- probably during the five years simulation there are many typical conditions of temperature inversions.

I don't know if Dr. Reiss got them, but probably would be more frequent in other areas from Florida and California, I think. But this is one thing that I think we need to be considering.

DR. ROBERTS: Dr. Wang.

DR. WANG: To comment on your first question, I personally have had the opportunity to work with Trical on the west coast and Hendricks and Dao (ph) on the east coast. I think those two companies, they are actually one, covers pretty much the whole country and goes through Canada and Mexico.

Their equipment for applying fumigants in methylbromide, chloropicrin in the past, now MITC and probably methyl iodide also coming on-line once it is commercialized, since their equipment tends to be standardized, although they change, but not in a very short duration, so that tends to have some stability in the short term, meaning years, three or five, before they change.

So that translates to the uniformity, you may say, of how those different ways they put on their shanks, their different equipment, will translate to the flux dynamics how the fumigants may come up. So those may be a very unique.

If you were to apply these on a large scale in the country, original basis, you may try to explore too their main equipment being used in major agricultural areas or forest nurseries and pick those as a representative case study and come up with a key scenario. So that will cover a much, much larger area.

Another point is that we have done some experiments in forest nurseries. This goes back to the comments by Dr. Yates. Those are very different from agricultural fields.

Those forest tree nurseries, they tend to occur in forest settings, but it has a small opening. So the meteorology is quite different. Although the overall acreage is smaller, but they put on quite a bit of fumigants. Those are going to be there for a long time.

That may be another scenario that you may want to include in terms of doing the PERFUM when you add another to a database, mainly due to the -- meteorology would be quite different.

The other thing you asked about a variety of fumigants.

There are other groups, MITC products like dazomet, or metamsodium. Those may not be done by the commercial applicators. Usually they are done by the producers. Application of those are going to be very different, the equipment and also the dynamics with the fumigant fluxes. Those may need to be dealt independently from some of the commercial applicators.

So I wonder if you thought about those things.

DR. ROBERTS: Mr. Dawson?

MR. DAWSON: Yes, I would say our thinking is very consistent with what you are describing. For example, with the nursery situation where we brought soil fumigation here as the case study, but we're keenly aware of other, what I'll call industrial sectors, where fumigants are used.

We're in the process, let's say, with our assessments that are ongoing at this point to look at those other industrial sectors and use the data that are available for those kind of different commercial and other settings.

Our plan is to -- and we would potentially apply this methodology or methodology of this nature using flux information that was specific to those kind of industrial sectors.

And as far as the other types of fumigants go, for example, I think you mentioned dazomet or MITC, we would be integrating in the specific flux information for those chemicals and trying to account for the broad nature of how those chemicals are specifically applied to account for the specific practices associated with them.

DR. ROBERTS: Dr. Majewski.

DR. MAJEWSKI: I would like to make a comment on the applicability of the source flux terms in other areas of the country.

When we measure field fluxes using the aerodynamic method, there is very specific field conditions that are required to increase our confidence in the flux values or to make the equations work. And that is flat, and it has -- the field, the source field is flat, and the area surrounding the study field is of the same consistency for a large upwind fetch. That's so you have a stable boundary layer that is developed over the test area.

Now once you start adding buildings or topography or windbreaks or anything that will disrupt that boundary layer development, then the flux equations break down and there is a large uncertainty in the source flux value.

In California or Central Valley, especially, it is like Dr. Seiber said, there are very few trees and it is very flat. In fact, they use lasers to make sure the fields are extremely flat.

So I think validating the source flux in an area that has -- it is like Iowa for it's undulating topography or some other areas or even around here where there are flat fields but you are surrounded by forests, I think the source flux term would have a very high uncertainty associated with it. Validating the output, the model output to the field results, would be problematic I think.

DR. ROBERTS: Dr. Seiber.

DR. SEIBER: Just had one follow-up comment. The comment about inversions, I think, is very well taken.

It is certainly a major factor in California in the Central Valley and probably in the coastal valleys too, because that helps you predict when you are going to have ground fog when the inversions.

The rule is you get up in the morning and it is foggy where you live, that's a good time to go skiing because it will be really nice up in the mountains. So you can just see that inversion has a really big impact both regionally as well as locally in given fields.

And I meant to mention fog in connection with the atmospheric moisture. Fog is a little different than rain. It doesn't deposit back to the ground, but it can still potentially be a sink term in downwind fate of chemical like methyl iodide or some of the other fumigants.

DR. ROBERTS: I think what I have heard from the panel in response to this is that the methodologies in the PERFUM model could, in fact, be applied generically. But the panel has identified several considerations that could affect the flux and perhaps also the dispersion depending upon the fumigant used in the local conditions.

Obviously, our minutes will reflect those various factors.

Is there anything in terms of follow-up questions or clarifications that you would like on this particular topic?

MR. DAWSON: No, I think if it is written from that perspective that will really help us.

DR. ROBERTS: Dr. Reiss?

DR. REISS: I have nothing further to add. I generally agree that the more we can understand the variability of flux with location, that's going to be very helpful. In terms of meteorology, we focus on California and Florida because they are the major use areas for this particular product. But there is a plethora of meteorological data out there that can be used within the model to look at all those variabilities.

DR. ROBERTS: Right, and some of the things, of course, the panel brought up are theoretical considerations, and it remains to be established to what extent it might practically impact. But they are certainly worth considering.

Any other comments from the panel on this question before we take a break?

Then I have high noon. Let's take a break for lunch. Let's get back together in an hour at 1 o'clock and we can tackle the last two questions.

(Thereupon, a lunch break was taken.)

DR. ROBERTS: Let's proceed with the questions. I think, if I'm not mistaken, we are on number seven.

While we are making some adjustments to get ready to read number seven, let me ask the panel if anyone knows the whereabouts of the laser pointer that was up here at the end of the sessions last night, it apparently has vanished.

If you could check around or if anyone knows, the SAP staff would be most grateful if we can locate that. It's sort of a big brick-looking kind of pointer.

MR. DAWSON: Question 7, please comment on whether PERFUM adequately identifies and quantifies airborne concentration of soil fumigants that have migrated from treated fields to sensitive receptors?

The Agency is particularly concerned about air concentrations in the upper ends of the distribution. Are these results presented in a clear and concise manner that would allow for appropriate characterization of exposures that could occur at such levels?

The PERFUM model calculates the concentration distributions both in all directions and for only the maximum concentration direction. Can the panel comment on how accurately the model approximates both of these distributions?

DR. ROBERTS: Dr. Yates, could you lead off our discussion on this question?

DR. YATES: I look at this -- it seems to me that the first question that's being asked here is kind of similar to a number of questions in the other topics.

The way I was reading it was that it refers to the accuracy of the model for predicting concentration at a receptor and further for the development of an buffer zone.

And to try with the idea that people outside the buffer zone, you know, to determine the risk that they might be exposed to a higher concentration than what the model predicts.

And we have had a lot of discussion up to this point about some of the uncertainties in all this. And it seems to me that one of the inputs or at least the way that this model works, it assumes that the soil is kind of a black box that provides an input but without too much detail about what is occurring.

An interesting thing to me is that in most of my career I have been on the other side of it, where I look at the soil and the atmosphere as a black box.

As far as this relates to being able to develop a highly accurate model that can determine risk at the -- sort of the extremes, it seems like eventually you will have to move to a situation where the model is considered and the interface and the atmosphere. But that of course is more of a long term goal, I think. And so I'll just leave it at that.

It seems that if the input parameters are appropriate to the site and the time, that the model can provide information that can be used in risk assessment. It does give you that information in a probabilistic sense. And you do get information at the upper ends of the distribution.

I thought that the report was pretty clear in this. I mean, there are a number of figures in the presentation that Dr. Reiss gave yesterday, that it seems it is very clear and concise in how it reports the information. So in that regard I think the model does perform well.

As far as the weather, it is accurate -- as far as -- the other question I guess deals with the concentration distributions in all directions and then in the maximum concentration direction. To me, I don't know that the information is provided to know how accurate those measures are.

I think that would require some kind of post analysis where the model is used somewhere and then someone goes in later when the buffer zones have been determined and checks to see if they are reasonable or not.

And nothing I could find in the report really addresses that directly.

But it seems like it might be possible to take the flux data that's available and try to do some sorts of calculations in that sort of a manner.

For example, for the flat fume, there are two flux studies. I could see a situation where you could use one of them to parameterize the model and then try to simulate what happened at the other one and look at how well the model works.

The difficulty I think is going to be that you don't really have measurements out as far as the buffer zone probably will be predicted. But there may be still some ways to correlate model performance to what occurs at a site, at a different site.

And for the raised bed, there are three studies so there would be a number of combinations you could use, look at -- combine two studies to get averages and then try to test out the third site.

I think with that kind of information we would be able to answer this a little bit better. But as it stands, I would suspect that the model performs okay. But there isn't really anything in the document that would allow me to say that it is accurate or isn't.

So I think that's something that probably will have to be looked at in the future.

I think that might pretty much complete my comments.

DR. ROBERTS: Thank you. Dr. Maxwell?

DR. MAXWELL: I concur with what Dr. Yates stated. I just wanted to ask in breaking down the first part of the question, the quantification of airborne concentrations of treated fields to sensitive receptors, during any of the studies have there been use of portable air quality samplers?

DR. REISS: All of the studies have used charcoal tubes to collect the data.

I'm not sure what you mean. You mean like a real time measurement?

DR. MAXWELL: How many samplers have been out there in the field?

DR. REISS: Anywhere from 8 to 12, and then we break it into periods. So we're getting a kind of diurnal pattern as we go through the post application period.

DR. MAXWELL: What is the general distance from the closest to the furthest away?

DR. REISS: They range from about 30 to about 140 feet and surround the field.

DR. MAXWELL: They would all be projected downwind?

DR. REISS: No, we actually put them in all directions around the field.

DR. MAXWELL: The second part of the question about the accuracy of the model in calculating distributions in all directions, to kind of follow up in your answer, once again I think you brought this up in other questions.

The accuracy of the dispersion coefficients and how we go about determining that, I know you have stated before that you feel that they are within a factor of two, my only comment on that is I don't doubt that. I just would like to see a little bit more information or basically verification of that.

DR. REISS: There is a lot of literature that deals with the general uncertainty with the dispersion models.

It is very difficult to peg a particular number. Everybody, I think, is reluctant to peg a particular number to the uncertainty, the general uncertainty of the dispersion models, but I think this is substantially better than a factor of two. The factor of two may come into play when you are looking at a stack source emitting and you are talk about far downwind concentration. But the idea Dr. Hanna had of treating the dispersion coefficients as a stochastic variable is one way we can quantify that uncertainty.

DR. MAXWELL: Thank you. That's all of my comments.

DR. ROBERTS: Thank you. Dr. Ou?

DR. OU: I don't have much comment about this aspect, except I notice since we started the sample distance, generally the parameter is 30 feet and 140 feet.

Some did not have 140 feet and the buffer zone distance for five acre generally was over 500 feet. I think it would be a good idea to carry out, to have one experiment to have a sample distance more than 500 feet, maybe up to 1,000 feet.

So it will give you a more reliable data to confirm that your monitor is reliable concerning distribution and concentration.

DR. REISS: Partly we're relying on the reliability of the ISC model to deal with predicting downwind dispersion. But the experiment you described is in the planning phases where we will look at concentrations farther downwind. And we'll get a better idea of how the predictions work at that distance.

DR. ROBERTS: Before we move onto the next comment, I just had a clarification question as a follow-up to Dr. Ou's comment. At what point does analytical sensitivity for this compound become a limiting issue in how far can you go out? In other words, I know the 120 may or may not ultimately be the number but how low can you go with -- I know it is a function of the collection period, but with a one or a two or three-hour collection period?

DR. REISS: I can't fully answer that question. I have actually been charged with helping them design this study to get some advice on that. We have not completed that analysis yet. But certainly we can go out to 3 or 500 feet.

One hundred twenty is the number that we're working with at the moment and we can go substantially below that. If I remember, the detection limit is below one microgram per meter cubed.

DR. ROBERTS: Thank you. Dr. Seiber?

DR. SEIBER: As far as the question does PERFUM identify and quantify the downwind airborne levels adequately, there have been several studies used to validate the ISC as a predicted model.

It is not of course exactly the same, and those have mostly been done with methylbromide and telone and MITC. Some are published. Some are in a symposium book proceedings in various places. They should be available to help answer some of the questions.

But the key point is it does not appear that it has been really adequately shown with methyl iodide, so clearly those studies would need to be done.

I think you might have one study where you located downwind samplers and used the model to predict and then compared or am I wrong? Model predecision versus --

DR. REISS: That study is currently being designed where we'll have farther downwind estimate.

There is no reason why the ISC model shouldn't work for ethyl iodide. The only reason -- the major uncertainty here is whether we estimate the flux rate correctly. Once that material is in the air, I think there is enough experience with gasses like methyl iodide to rely on the ISC model to do that, to do the dispersion estimates.

DR. SEIBER: The only exception might be if a chemical underwent some deposition or degradation pathway that the others didn't, and then it might be different.

DR. REISS: That's possible. And if it did, then we would be overestimating the concentrations. I don't think that that's for methyl iodide, a significant factor at least when you're talking about a few minutes away from the field.

DR. SEIBER: That was my only comment.

DR. ROBERTS: Thank you, Dr. Seiber. Dr. Small?

DR. SMALL: My comments on this are brief. I think they repeat some of the things we have already addressed in some of the other questions concerning the uncertainty and the emissions. I think that air transport model is fine as well once it is up in the air.

I raise some of the -- again, if you are interested in the extreme conditions, again, how you treat the calm wind periods, I think is important even for a model that has been verified for regulatory use, it doesn't necessarily mean it was focusing in on those extreme conditions. It may have been more for longer averaging periods or different kinds of risk scenarios.

There is one point I did want to raise. I don't know if this is the right question to raise it here or the next one.

I'll raise it here. Again, about the high end and the protection whether or not you use 90, 95, 99 that's a risk management decision, but there is one technical aspect of it that you should keep in mind.

And that is if you are considering this as being protective in many applications at different locations, so if there is going to be 100 of these things done per year in the state of California or in the United States, presuming there they are independent, if you want to be 95 percent sure that you are not going to have a serious exposure in any one of them, for the individuals ones you have to be something like 99.9 percent sure on each individual one.

If you're -- it is a basic probability calculation. So just keep that in mind in terms of, you know, if this is a 95 percent calculation that's done for one location and you have to think about is it something that's going to be occurring frequently, how frequently, things of that sort.

DR. ROBERTS: Thank you, Dr. Small. Dr. Winegar?

DR. WINEGAR: Responding to the first part of the question here about the adequacy of PERFUM to quantify airborne concentrations, I base my evaluation on not so much personal experience with the model, but the fact that ISC has been vetted and validated by many other users in lots of different situations.

That combined with the fact that the indirect flux method, which is a -- I consider it kind of an analog to a laboratory calibration, you have a calibration input and a response and you look at that to judge what your further calculations are going to be based on.

So that appeals to me from my background as an analytical chemist primarily.

So in terms of the accuracy, how ever accurate ISC can be shown to be, I would presume that PERFUM would follow along the same lines.

Any questions I have in regards to accuracy would have to do with things that have been realized by others and myself in regards to things like the met data question, the different locations, one location versus the other.

I look at slide number 74 in your presentation where you did this percentile distribution of the different buffer zones using the different types -- different data sets.

And if you throw out, for example, the FAWN data sets from Florida and look at Merced on down to Santa Barbara, there is not a huge difference in the size of the buffer zone, 580 versus 680 feet. That's only 140 feet difference.

But if you look in the actual situation out in the field where people are doing these fumigations and where the fields are located in relation to sensitive receptors, 140 feet is pretty important.

I know on an almost block by block basis three of the major growing zones in the area, because I had to canvas them to find appropriate sampling locations for this methylbromide monitoring project I was involved in, and the land use is pretty tricky in a lot of areas because you do have residential areas and schools really close to a lot of agricultural usage areas. And so the 140 feet can be a big difference there.

And so I don't know whether the ultimate decision in regards to the selection of a met data set to use or whether it is regional or going to be one master data set or whatever. You know, if that uncertainty can be squeezed down more that would be, I think, a great benefit to the entire process.

In regards to the second question about upper end concentrations, again it comes out to the same kind of thing with the accuracy of the ISC model in general.

I also made a comment earlier in regards to looking at the output of the model and how it is examined. Again, if I refer to my personal experience with looking at on a really micro basis the different neighborhoods and the growing zones and such, being able to visualize a particular field and how the buffer zone may impact a -- put into say, a GIS type of graphic situation, I think it is pretty useful to be able to visualize how this the buffer zones can impact different areas.

I'm thinking in particular in Camarillo, there is a couple major fields that I saw, personally, fumigated a couple times over the course of a couple years. They are right next to Route 101, so the buffer zone would cross over 101.

I know that's a question that DPR has dealt with methylbromide I don't know how that will figure in the future, but that's just an example of how even a relatively small buffer zone in less than 100 feet can really impact how that particular field might be used and how it might affect particular receptors.

So I would encourage somehow taking that graphic output or taking that output and translating it into a graphic way. I think it would help the eventual users who are developing buffer zone being able to visualize and relate to the physicality of the situation.

I think a model is great, but when it just comes out a pile of numbers it is difficult to relate to physical situation. Any way that can be more directly related to a physical phenomenon or physical arrangement I think would be very beneficial to all involved.

Then the last question in regards to the all direction versus the single direction in the relation to maximum concentration for the wind direction, I think it is good that you have both options available.

I can't comment really specifically on the accuracy of those two different things, but again, the ISC model presumably is predicting accurately.

I would encourage in any further studies that you keep that in mind, that one direction versus all directions aspects in the layout of the samplers, so that maybe that question could be addressed.

And the last as a kind of a more field sampler guy, in relation to the field studies and this also relates to the calibration question and the accuracy, I would recommend -- I think you mentioned you were going to do this, additional rows of data collection outside of just one ring encompassing the field. More further down field I think would be useful to be able to more fully characterize the entire process.

DR. ROBERTS: Thank you.

Dr. Yates I believe has another comment?

DR. YATES: Yes, there is something I was going to ask.

Did you in part of looking at sensitivity, did you by any chance try the case where you take the flux from one, say one of the flat fume studies, and then use it as input for the other one with the meteorological data for the other one and look at differences between the buffer zone from the using the flux from the other study with the straightforward analysis where you actually back calculate it?

I'm not sure I'm saying that very clearly.

DR. REISS: I think I got the gist of the question. I could almost answer that in my head because there is a linearity between concentration and flux.

For example, with one of the drip studies we got 42 percent of the emissions in the first 24 hours. And then in the other one we had 50. So it is really the ratio between those two that -- ignoring the diurnal variability.

DR. YATES: That's the part that I think makes it kind of important. Because in some of the flux distributions, things came out at a later time and the weather conditions would be later. And it might have an effect on the complete analysis.

DR. REISS: Yes. I mean it would be more problematic to do that because the timing of the application and various local conditions are affecting that diurnal variability.

DR. YATES: Right. But in essence, if this is going to be used in a regional sense it is going to be that kind of translocation that's going to occur. You will be using information from one site under certain conditions in a different environment.

It just, I mean, it is the only way to really answer the question. And I kind of started thinking that maybe in the sensitivity analysis you may have already looked at that at least to some point.

DR. REISS: Yes, we certainly compared what we got with the flux profile with the various application methods, the profiles derived from the same application method in different studies. And there were some differences.

We're going to have to look at that when ultimately regulations are developed. We're going to have to look at the variability you get from those different profiles and some policy and scientific decision will have to be made to quantify that and determine what would be the most productive.

DR. ROBERTS: Dr. Baker.

DR. BAKER: Rick, could you clarify the linearity between the flux and the concentration at a point as opposed to the buffer zone distance? Because you have been mentioning linearity a few times. It might get --

DR. REISS: Sure. The buffer zones would not be linear as a function of the flux rate. It depends on the geometry of the calculation. But the concentration you would observe at a given receptor is linear with the flux rate.

DR. ROBERTS: Dr. Wang?

DR. WANG: Another clarification, in the report, the brief summary report on Page 11, on the table you listed all the concentrations that you measured.

I assume it is all measured at the same height or translated to the ground level?

DR. REISS: These are calculations made by DPR, I'm sorry, EPA, so they could comment on that.

DR. ROBERTS: Can you make it clear what you are referring to? It is not clear to the rest of us.

DR. REISS: This is on the EPA background document on Page 11, there is Table 4, ISC calculated air concentrations and selected distances downwind for pre-plant agricultural fumigants.

Basically, these are calculations made with simplified meteorological assumptions, for instance, one meter per second wind speed stability D, and so on. It is not using historical meteorological data, but just a comparison of the difference you would get with making various simplified assumptions.

MR. DAWSON: That's our current method and the receptor height is meter and a half.

DR. WANG: That's also the height of the measurements that were taken for those experiments that you have conducted?

DR. ROBERTS: Since the point hasn't come up from other panel members, I would like to comment briefly on the clarity of the results and simply to make reference to a previous comment by Dr. Portier that arguably there is some ambiguity in the results to the extent they represent variability versus uncertainty.

Other comments, responses in this question? Dr. Portier.

DR. PORTIER: Something came up as I was reading this and thinking about the last question here, which deals with accuracy.

No, that's the second question that deals with accuracy. When you think about it, accuracy addresses issues of bias. A lot of the things we have talked about is bias. But then there is also precision that goes with it as well.

And really to the uncertainty statement I made previously addresses the precision component. And I'm assuming once we get the right model hopefully that will be accurate. I just want wanted to clarify that.

DR. ROBERTS: Okay. Let me ask the Agency, then, are there any clarifications sought on the panels' responses or any follow-up questions on this topic that you would like the panel to address?

MR. DAWSON: Just a couple things. One, it is worth noting that with the -- just for some context, I guess, and perspective DPR had gone and done more or less an evaluation of the efficacy of their buffer zones, and correct me if I say anything that's not exactly accurate, and essentially there were 34 studies that they looked at for field fumigation.

They used essentially the same methodology in many ways as this model does. And what they found was on 33 of those 34 analyses that the buffer zones were indeed protective at the 95th percentile. Is that correct?

So I mean there is some sort of --at least a start for looking at validation types of analysis. And we look at that very closely when we started thinking about how we're going to do this and considering the methodologies that were developed by DPR that we based our analyses on.

I was also wondering if there are any specifics related to characterization that we really need to think about as far as language, anything that could potentially could be added, a big ticket item type of thing that would be helpful when we go and try to explain these results to people, you know, the uninitiated that don't have a background in this area, is there some recommendations that you can potentially make with regards to that kind of thing?

For example, the one graph that Rick and I, Dr. Reiss and I both showed, was that the kind of explanation that is appropriate or is there more detail, for example, related to those kind of things?

DR. ROBERTS: Let's see if the panel members have any thoughts about that.

DR. BAKER: By the one graph, you mean the one-day simulation?

MR. DAWSON: Right, the red and black.

DR. REISS: One idea while I have been listening to all the discussion is I could possibly add a function to calculate the buffer distance for each spoke and calculate the 95th percentile for each spoke.
And that way when you have done your run, you could plot that. One of the reasons we didn't give a lot of plots is because I could generate 1,800 plots for every day for a five year run.

But that might be a way I could summarize the results in a way that could be plotted in server or GIS program and would show the contours of the buffer.

DR. ROBERTS: Any other comments or suggestions from panel members on this point? MR. DAWSON: Great. Thank you.

DR. ROBERTS: Then in that case let's go ahead and take question number eight.

MR. DAWSON: A sensitivity and uncertainty analysis has been conducted and is described in the PERFUM background document.

What types, if any, of additional contribution or sensitivity analyses are recommended by the panel to be the most useful in making scientifically sound regulatory decisions?

What should be routinely reported as part of a PERFUM assessment with respect to inputs and outputs? Are there certain tables and graphs that should be reported? What types of further evaluation steps does the panel recommend for PERFUM?

DR. ROBERTS: Dr. Baker, could you start out our discussion on this one?

DR. BAKER: I think we have touched on a number of the issues on the sensitivity and uncertainty. But let me go through them because there were quite a few.

The flux we talked quite a bit about. In the modeling framework you have the opportunity of perturbing that, we talked and different ways of doing that or not perturbing it, we have talked and possibly a regional or state specific flux to account for a number of environmental factors.

In your discussion, you do mention a number of the environmental factors that would come into play exclusive of the met. But, again, for right now we just have a limited field study.

It doesn't warrant a thorough investigation at least from these field studies. It is not statistically significant as you've mentioned.

The meteorological sensitivities you have looked at and have included the anemometer heights from the field studies, but also just to the nature of the structure of the PERFUM model you are looking at the variants of the meteorology by looking at 15 stations originally, and then taking a station from each of the corps tiles. So you have a representative set.

Within the meteorological preprocessing issue of handling the calms it is according to the ISC approach. If something better comes along, if there is other alternatives, I don't know what they would be, but that's something to consider.

By reference of other work you mentioned model to model comparisons, for instance, ISC to AERMOD. You have looked at indoor exposure and time away from this site as well. So I think you had a total of four scenarios there for indoor slash activity pattern.

You have looked at using the model for multiple field applications. In the report you mentioned it is hard to generalize unless you know the extent of the buffer zone.

Certainly, the longer the buffer zone, the more potential there is for overlap. I'm familiar with some cluster analysis for air toxics in urban environments and surprisingly there isn't a large impact predicted from ISC, at least in the constraints of ISC.

I don't know how a different model operating with different meteorological inputs, say, MM5 would handle that, at least for ISC it is not surprising.

You have looked at seasonality. We did talk about possibly slicing the data some other ways looking at particular months for different years within the different seasons, was one way of looking at it.

I think the breadth of the sensitivity and uncertainty that you looked at is commendable, and while there could be some details as to how to do it differently, it is good to see everything is laid out in the report and people can fairly judge. And where they have their particular areas of expertise they can comment on that.

From a more global perspective, I think the amount of data seems to be in line with the dozen or so field studies that have been conducted for ISC calibration. And the number of permitting health as well as socioeconomic decisions have been based on the modeling.

So I think for comparing decisionmaking quality of data supporting decisionmaking, I think it is reasonable. I'm not conversant with how much calibration there has been in say a regional model like Calpuff.

Photochemical models are being used to make a number of health and socioeconomic decisions. There is not a lot of field studies the supporting that.

There is environmental fate models like TRIM. Shortly, I think health and socioeconomic decisionmaking will be based on environmental fate models. Again, not a lot of field studies.

So the fact that there are places where it can be suggested, additional studies would be useful, it is not as if these absence of complete data has limited decisionmaking in other arenas.

So I think what you have done is certainly on power with what I have seen supporting other decisionmaking processes.

One variability we have talked about a little bit is chemical -- different fumigants certainly for volatile fumigant that are not highly reactive, ISC will treat them the same.

When you move into other chemicals that might have other properties you discussed, those would have to be considered on a case-by-case basis.

Such as if there are chemicals that lead to deposition products that would decrease the downwind field, but it may lead to considerations of environmental fates, other routes of exposure. It gets complicated in that facet.

We have talked about the time of day seems to be important consideration. You have that factored in. The periods, the blocks of hours that ISC takes at an nominally constant emission flux rate is determined by the field study and the sampling at the field study.

I'm not sure how you might perturb that. But we did discuss some potentials on perturbation on that.

And it is good to see you have an additional field study where you have additional distance to the monitors for the field study so that you can test your flux calculations at different distances to get the highest concentrations you would like to be close in. And presumably that's what the initial field studies focused on, is close in.

You also would like a large enough distance source receptor, in this case, source to your carbon canisters for the meteorology to have time to have a reasonable impact that when you do your back calculation you are getting numbers that are supported by the prevailing meteorology and maybe not some other local phenomenon.

And one of the questions was additional information and it was mentioned in the interface user -- input output interface is nice but maybe not necessary.

And the possibility to overlay once you have ported the imported the data into a GIS system, overlaying it on some maps. I'm not sure how extensive is the databases on maps for rural areas as it is for urban areas, but maybe that's something you could comment on.

DR. REISS: It certainly would be possible. If you are looking at a site specific application, we routinely do that for other applications where you get -- it is almost all free. You can download satellite maps and overlay in GIS the contour plots. That certainly would be possible.

DR. ROBERTS: Thank you, Dr. Baker. Dr. Hanna?

DR. HANNA: Just to add to that, one way for me -- if it is possible really to isolate the uncertainty for meteorology from uncertainty related to emissions just to get a feel of which one is contributing how much to the uncertainty and of course the concentration and also the buffer zone and sequencing on the exposure.

And really one way to do that is, for example, to run the ISCST3 model with the same meteorology for a certain station or for a certain location. Not necessarily for five years, but even for one year or so just to get a quantification, but with what we call base case.

And then actually perturb the emission pattern based on a certain uncertainty distribution that can be detected or can be calculated for this type of -- for certain type of emissions and see how much difference between the two cases can give in the buffer zone and of course the concentration and this kinds of stuff.

DR. ROBERTS: Thank you. Dr. Small?

DR. SMALL: Once again, I think we have discussed some of the key issues. I think that in terms of the implementation that you should consider changing for your sensitivity and uncertainty analysis in terms particularly how the uncertainties for different conditions and different sites would be generated on the emission rates.

I think the overall general discussion and approach is good. It is very clear, so it is easy to follow.

I'm going to follow-up on a question that Dr. Wang asked yesterday. Maybe you would have asked it again later. And that is the idea that you did look at individual, the sensitivity to individual aspects. You haven't done really an integrated uncertainty analysis.

I would not recommend doing that now. I think that the need for improvement in some of the structural assumptions in the current model are such that it is too early to do a full-blown uncertainty analysis at this stage.

I think you need to work on some of the

structural aspects a little bit more first. Then I think that eventually you will be able to do an uncertainty analysis on the uncertainty parts of the model.

So I would hold that off in the future and maybe even until you get some more mechanistic aspects into relating emissions into atmospheric and soil properties in some way as well as mass balance constraints using some of the approaches we talked about yesterday.

The other issue that might come up, again, I don't know whether this is a technical issue or a risk management issue, is to what extent upset or unusual conditions that could lead to an especially high exposures ought to be considered.

This would even go beyond the calm wind issue. But just in terms of the way applications are done or things of that sort. I don't know if that's -- if there are things of that sort in terms of just spills or improper laying of tarps or things of that sort that come into play.

Again, if you have got something that's constrained by 100 percent mass, then you are able to put a pretty reliable upper limit on that. But I think those things might be considered as well.

That's it.

DR. ROBERTS: Thank you. Dr. Spicer?

DR. SPICER: At this point I think that a lot has already been said. I will simply end up repeating.

But none the less, with regard to sensitivity, I think that this idea of setting CV equal to zero as far as the flux is concerned is a valid suggestion.

Considering constant met conditions with the flux variations to actually separate those two effects as far as the uncertainty analysis is concerned, I think it would be helpful in understanding exactly what the model was doing.

As far as the flux measurements are concerned, the only thing that you might want to consider that has not been talked about a lot is looking at faster response concentration measurements in addition to the vertical concentration profiles and that sort of thing.

What I see is a situation where with the concentrations being averaged over a longer period of time then you can actually be getting agreement for the wrong reasons. There can be some wiggle room there that I think faster response measurements, even an hour response as far as that is concerned, would be more beneficial than is done with the two to three hour time that's presently used.

I think that the point has been made earlier that modeling the flux since this is going to be diffusive phenomenon through the tarp and the membrane, is a valid approach and should be considered. Because that eliminates this issue of mass balance problems that you can have. You are certain of evolving all the mass, because you are modeling it so that you evolve all the mass.

I think that the consideration of the film thickness when characterizing the flux is important. And recording other parameters such as the soil temperatures, even insulation, because what you are going to be looking at there is potential for the soil to be heated up in the vicinity of the tarp, which changes the diffusivity and those sorts of things.

Although you may not have enough information at this point in time to sort out which one of those characteristics are important, obviously with the methylbromide program you have been in a situation where you have had additional experimental trials and it is possible that those sorts of effects could be sorted out if they are recorded at this point in time.

And then the last thing, of course, that's been talked about quite a bit is this idea of the calm conditions.

In both the issue of incorporating those in the flux measurements and also the exposure limit determinations, I think that those are still open questions as far as that's concerned. I understand the comment that you made earlier that ISC was -- that issue was probably addressed in the validation of ISC.

But the other thing, I think ISC was not necessarily meant to model concentrations in the very near field. And that's I think more of what we are looking at here as far as application than may have been originally intended as far as validation efforts were concerned.

And then the other point is that there are models available that would allow to you start considering those questions other than ISC.

DR. ROBERTS: Thank you. Dr. Winegar, would you like to add some comments?

DR. WINEGAR: Yes, just one short comment in regards to the need for what I'm calling the black box versus the more fundamental physical type of model.

A lot of mention has been made about more sophisticated models that would take into account soil moisture and carbonaceous content, this and that.

While I agree that may be advantageous from a purely scientific standpoint it seems to me there are a lot of uncertainties in that whole approach. I don't know how well that science is all developed in terms of the problem that we're looking at here.

And so I wonder whether the black box approach or the indirect calibration may be actually better than trying to do a more fundamental detailed physical model from a soil basis.

And so I have just a gut feeling. I don't have anything other than that to back it up, that the uncertainties involved and the more fundamental model would be greater than what would be involved in a well designed indirect field study.

DR. REISS: I agree with that comment. We have between the seven studies we have done, between 35 percent and 61 percent of the applied mass was evolved during the first 24 hours. Part of that difference can be explained by the application method.

We would need a model that was good enough to further explain some of that variation, like the variation between Camarillo and La Selva Beach drip was 42 and 50. You are getting to small differences, which I highly suspect that these soil based models are not ready to account for, at least for this particular chemical.

DR. ROBERTS: Dr. Spicer?

DR. SPICER: I don't necessarily disagree with that as far as that's concerned. I'm just suggesting that that's the more fundamental approach as far as the flux modeling itself might be beneficial.

Now, as far as this issue of this idea of calibration in terms of looking at the fact that you have treated a field and you are looking at the concentrations at a single level, the problem with that is that, unfortunately in my opinion, is not calibrated.

The simple reason for that is the atmosphere is not a gas chromatograph in the sense that what you put in doesn't come out in one spot. It's coming out in several spots.

What your concentration measurements are indicating is that there are times that the model is not correctly predicting where those spots are. That's the fundamental issue associated with this calibration idea.

All I'm suggesting is that you can go a way long to curing that issue by simply looking at vertical concentration profiles in a way that allows you to better see how the model really does compare. That's all I'm suggesting.

DR. ROBERTS: Dr. Wang.

DR. WANG: I'm not really trying to defend the soils model. But it does have the capability to treat different application methods, either you use drip application or shank or sprinkler service applied.

There is different ways so we have two dimensional, three dimensional models to treat those things. Where the source -- we call that where you apply injected, will be described in either two dimensional or three dimensional grid. That has been done. It is not new.

Also, those models have capability to do, as Dr. Ou has shown already, simultaneous heat transfer, water flow and chemical transport in both dissolve portion of the solution phase and the gas phase. So it has the capability. It is just, again, probably hasn't got that far to be utilized as a regulatory tool here.

It is intensive computation wise. Also probably requires some more background in science and modeling.

DR. ROBERTS: Dr. Bartlett.

DR. BARTLETT: I think it may be that there is problems as far as the development of the soil models go. But I think that they would still be useful.

I would like to just comment on several aspects in that, my experience with air transport environmental fate, a lot of people accuse this type of analysis of introducing compounded uncertainties.

But when we did our sensitivity analysis and uncertainty analysis, we found that we generally would be reducing overall uncertainty by adding in other factors. And our validations have improved and this has been true of other people doing the same sort of work.

But this is air, not soil modeling. I believe incorporating and using some of those other parameters in the modeling in a direct or indirect way might be beneficial. I think the area that it's most important is if you are going to generalize the model to use it to apply to situations with different soil types, temperatures, the different factors that we do have an understanding, do affect emissions.

So even if it is a question of understanding how the model applies to situations that may affect ultimately the boundary buffer zone by affecting the rate of emission on the first day, some of these factors and understanding more about them. So as you do more studies to keep, to take records of these types of information that are in the soil models that may -- some of them may or may not have as much to do with some of this particular substance but it sounds like for methylbromide that there is enough correspondence that what has been learned from there could be applied to there. But if it is going to be generalized to other chemicals then some of the other information may become valuable too.

DR. ROBERTS: Any other comments in response to this question? Dr. Portier?

DR. PORTIER: I was thinking about the question of additional inputs, outputs, tables and graphs.

When this model becomes really ready to be put out, I think we have to be very careful to identify what inputs are constant, what inputs are variables, what inputs have uncertainty -- parameters with associated uncertainty.

It was unclear in the document when you considered something a variable. So for example, a lot of the climate variables are actual variables. Flux is a parameter with uncertainty.

I think it would be nice to be able to look at some of the variable inputs and talk about how different decompositions of that variability may work its way through the model.

For example, we talked about time of year. You could also think about what crops is this chemical going to be used with and what are the critical times of the year for that crop.

If these are strawberries in south Florida, I want to know what is happening in December, January. I don't care what is happening in July, because I'm not going to be fumigating my field in July for strawberries that I'm going to harvest in February.

I think when it starts to get used there may be some very time-specific components that need to be passed through the whole model. And the final output reflects some of that stuff.

There are other model parameters, that way I would look at it, that we need to look at a full sensitivity analysis. I agree with Dr. Small, you are not ready to do the full sensitivity analysis. But there are some parameters we talked about in addition to flux rate it would be very easy to say how important is this calm, not calm factor. That's something that we can put in perspective with the uncertainty from flux rates. If it is very small, then we have wasted a lot of discussion, rural versus urban, terrain issues. That's the simple terrain issue.

But you can certainly run the model in both scenarios and see what the impact is and tell us that.

That's what I would expect in the way of modifications to input and outputs in a more final document. It is something you need to be thinking of as you develop this.

DR. REISS: I have run the model or at least part of the model with the urban and rural options. It is about a factor of two difference. So the rural option gives about a twofold higher concentrations.

But those are great comments.

DR. ROBERTS: Any other comments in response to this question?

Let me then ask the Agency if there are any clarifications or follow-up questions they would like on this topic.

MR. DAWSON: No. Thank you.

DR. ROBERTS: That completes our discussion of the questions posed to us by the Agency.

I indicated to the panel before we started our deliberations that I would give them the opportunity if there were related technical matters that they thought should be brought to the Agency's attention they would have the opportunity to do that.

I would like to do that now. Let me then open the discussion to the panel if they have any other comments or suggestions regarding this model related to the topics that we have discussed. If they would like to broach those. Dr. Baker?

DR. BAKER: I guess we have talked a little bit about the analytical portion of the field studies. And having a little bit of background, are there any potential if not immediately available on the horizon techniques for quicker response maybe even to eliminate the back-calculation method, something that could scan the surface and get an actual flux reading, any techniques like that?

I don't know of any. But if there is anything interesting that you could think of that you could include, that would be helpful for just a general perspective. If not, just explaining the canister method would help to clarify a little bit what was presented and the time blocks for the flux that was chosen and for the modeling.

DR. REISS: I'm not aware of a method that you can get the flux estimate in a better way than the indirect or direct method.

I don't know. Jim, could you comment on the canister methods? Jim Platt is a chemist that directed many of these studies.

DR. PLATT: Jim Platt, with Arvesta, really when we went into these programs we looked for the best technology available. And after looking at various methods, these -- we called them canisters but these are the cylinders that the glass samplers would have a front and back portion so you measure what is being collected.

That's by far the best thing that we could find. I have heard about other techniques during the SAP discussions here that we're going to look at. But that's really -- and then we have to get something that's acceptable to the Agency as validated and reliable.

We're looking, but right now the carbon, glass cylinder containers seem to be the best samplers.

DR. ROBERTS: Dr. Winegar and then Dr. Spicer.

DR. WINEGAR: I can comment on that question. There is a very well used and very well validated method called, actually, Suma canisters. It is an actual canister and evacuate sphere. It is used extensively for all manner of EOC sampling. Literally hundreds of thousands of samples are done a year across the country.

This technology allows you to do anywhere from a grab sample of just 30 seconds up to an integrated sample of according to one manufacturer, up to a week.

I don't know how reliable that one is. But at least up to 24 hours is very reliable. Anything in between is possible.

So that's an alternative to the charcoal absorbent approach. It is probably more expensive frankly, but can give something that the absorbent lacks.

DR. ROBERTS: I think the advantage to the charcoal method is you can move larger volumes of air through and collect material from a -- analytical sensitivity I think becomes an issue in terms of the collection approach as well.

DR. WINEGAR: Not really. You can get to -- commercial laboratories get down to .1 PPB routinely without great expense.

DR. ROBERTS: Dr. Spicer, I believe had a point.

DR. SPICER: I just had a question. Where the charcoal cylinders aspirated or not?

DR. REISS: I don't know the answer to that, maybe Jim does.

DR. SPICER: Do you draw air through them?

DR. PLATT: Absolutely. The procedure was of course to go into the field and establish background levels with these etermasts (ph). Then the charcoal tube had an automatic pump with it that was going, I think, 58 miles a minute and those were running continuously. That was the aspirator.

Is that what you mean? That's how those were done. Periodically calibrations were checked and rechecked as ran through that.

DR. ROBERTS: Thank you, Dr. Platt. Dr. Seiber?

DR. SEIBER: I think as far as here and now, major change -- of course what you really want is something that could give you a reading out in the field, an insitu method. I know FDIR was tried. I don't think that proved cost-effective or sensitive enough.

Long pathlinks spectroscopy of one type or another might be used. I don't know. But here and now, I think you are talking about moving portable gas chromatographs and things that could be taken out to the field to process the samples right there more quickly.

And I don't know whether anybody has done that. But this would help somewhat.

DR. ROBERTS: Other comments? Dr. Portier?

DR. PORTIER: I was thinking about the overall sampling design and its relationship to the flux method. If you think about and I don't -- I think there is some room here to play around with the model to figure out where to put this grid to kind of improve the power of the back-calculation methodology.

You placed your samples on the edge and then corners further out. There is no guarantee that's the best design. And yet with a little bit of playing with your model you actually might be able to find a better configuration with the same number of observations, say twelve observations, that will give you a better way of fitting that regression, which will improve the fit.

And that doesn't require any -- that just requires playing on the computer a little bit, being clever and thinking about how to use that model.

DR. REISS: You are probably right. One of the concerns -- the reason you have it in all directions is you could have a predominant wind direction. But it is just that, a predominant wind direction. It is not exclusively in that direction.

You certainly don't want to do a study where you get zeros and get no information left. You really need to circle the field to some extent just to be safe that you are going to have a reliable measurement you can use later.

DR. PORTIER: I recognize that, but I'm still saying there may be a better configuration than having everything on every edge.

The other thing is we sometimes think we have to use the same methodology every where. And there may be a cheap and expensive method, if you could intersperse those you can use a combination of the two. It sounds like everything you have been dealing with is expensive, so I hate to bring it up until the very end here. Hopefully there is something like the gas spheres that may not move as much air which allows you to analyze the concentration to a better level of accuracy, but can be cheaper and placed in more directions to give you a second level kind of fit to your model.

DR. REISS: It is possible. It is possible that there is a method out there that's not quite as accurate as what we're using, but if it is cheaper and we could deploy more samplers you could do an analysis to show it could be more accurate in terms of back-calculating the flux.

DR. PORTIER: If you can put the cheap one and the expensive one together in the same expensive spots you can do the calibration between the two. There are some clever things, if you get more money and more time that you can do. Right?

DR. REISS: Thanks.

DR. ROBERTS: It always comes down to money and time, doesn't it? Any other issues? Dr. Seiber.

DR. SEIBER: You mentioned -- I think somebody mentioned ways to measure flux out in the field. That is an issue here. We have a back-calculation method. We have various versions of the aerodynamic flux method. And it seems like it would be good if some agreement was reached, particularly as the model gets to be extended to other parts of the country, on a standard method or at least an agreed upon method.

I just ask one of our panelists here, Mike Majewski, I know he has had some experience with a single height measurement and then also I think, Mike, there was a downwind a vertical and a horizontal flux method? Do those offer any improvements?

DR. MAJEWSKI: The problems with those methods is they are all based on the same assumptions. I prefer the aerodynamic gradient method that they used and that DPR used, because that gives you a picture, an actual picture of the concentration of gradient. Whereas the single point method gives you a single point. And if something screws up, you have lost that data point.

The vertical profiles and the horizontal profiles are -- well, the horizontal profile, wait a minute -- they are basically gradient methods as well. They will give you a picture of the concentration gradient either with height or with downwind distance.

Again, they are based on the same theories. So they should in theory, give you the same number. But as Dr. Yates pointed out in his slide, the period variation can be substantial between these methods. But the overall cumulative flux seems to be in good agreement.

I would recommend staying with the aerodynamic gradient profile method. That probably gives you the most bang for the buck.

DR. ROBERTS: Dr. Winegar?

DR. WINEGAR: I would have to agree with that. I have been involved in a lot of different flux measurements, primarily using the surface isolation flux chamber and that of course has its limitations for this situation, because it is very difficult to go onto these beds and to use that kind of a flux chamber for this kind of a situation.

So from my experience, I think the indirect method of placement around the field, et cetera, is probably about the best you can do.

There are, in my opinion, no sensitive enough technologies available to do like across an open path type of measurement. Those typically involve 1,000 meters to get the highest sensitivity. Even at that you can maybe get down to double digit part per billion. You just don't have the sensitivity to be able to did that.

There are portable instruments that can do very rapid analyses. Unless you had a number of them you wouldn't be able to do each one simultaneously, you would have to do them sequentially and you would probably separate by maybe 10 minutes. I don't know if that would be acceptable to put together into the entire experimental design.

So from my standpoint of how to obtain flux, however imperfect this method may be, it is probably the most practical thing that can be done currently.

I think the gradient methods do have some appeal, but the input or the constraints to the field situation are pretty severe as I understand it and would really severely limit the number of locations that could be tested.

So from a balancing everything both scientific and pragmatic considerations, it is my opinion that the indirect method is about the only way to go.

DR. ROBERTS: Any other thoughts by panel members in our open discussion?

Let me then turn to the Agency and ask you if there are any aspects in either this last discussion that you would like to have clarified or in the course of our discussion over the last two days there is some follow-up questions that you would like to ask, pose to the panel?

MR. DAWSON: I think we're fine on this

discussion we just had -- Dr. Barry from DPR had a question as a follow-up from yesterday about the calculation of flux alternative.

I'll turn it over.

DR. BARRY: Terri Barry, DPR. Actually this segues well from what Eric was just talking about.

Given that we have a large data set for methylbromide, 34 studies that we know we can back-calculate and we have seven studies iodomethane. We have metam studies, which by the way, the direct flux might be difficult with metam because you water at intervals, there are problems with water on charcoal samplers or any other sampling method.

So given we have this large database, I'm not sure that I really got the take home message of how we would do the back-calculation statistically so we get an accurate measurement of flux given the data that we have.

The questions are force of the origin or not, log transform or not. We kind of got into that discussion, but I don't really feel like we got the guidance that I have a take home message on. And how you do it can make a big difference. It can be as much as 45 or 50 percent difference in your flux estimate.

Can the panel address that a little further?

DR. ROBERTS: Dr. Small?

DR. SMALL: I'll start. Don't log transform, that violates the mass balance assumption. Decide whether or not you think that there is a physical reason for there to be drop, that there would be concentrations above zero had that field application test not taken place.

If there are, then don't force it through the origin. Because then it is a real background concentration.

If there are not reasons -- if you wouldn't -- if there is no reason for there to be some background concentration, then go ahead and force it through the origin.

The question I thought you were going to ask is whether or not the methylbromide data can be used to make inferences about other chemicals, the iodomethane. And you don't want to ask that one?

DR. BARRY: No.

DR. SMALL: When you get to the point were you have a little bit more of a physical model that has things in it like diffusion coefficients of the gas or whatever it has in it, you're going to be able to start borrowing information across tests for different chemicals.

The way it is set up now each one has to stand on its own. I think with a more physical model what you learn about one chemical will be transferable to other chemicals.

I guess that's something that's down the road.

DR. BARRY: That was not my question. We're assuming it is chemical by chemical with the back-calculation method.

DR. SMALL: With the back-calculation method that's all you can do because it is a variable.

DR. BARRY: One more comment on that. I think what I wanted to make sure of is that we are using most effectively the data that we have for each chemical, that we're making proper use of it.

DR. REISS: Dr. Small could I ask you a question?

DR. ROBERTS: Before we go on, I think Dr. Portier also wanted to comment in response to that question.

DR. PORTIER: On the back-calculation method, the problem here is that the regression is an empirical method. It is just describing the relationship between those factors.

And you have got two models here that are not -- one is a two parameter model. One is a one parameter model. And without a mechanistic reason to choose between the two, they can both be fit perfectly well from a statistical point of view.

Now, I agree the flux measurements can be quite different. But we don't have any independent way of deciding which is better from a statistical point of view we say it is a good model, it describes what goes on.

Until you can give us more physics, more reason to choose one model over the other or to basically lay down the law and say it absolutely has to go through zero forever and ever and it is linear in the short range, which is really what you are -- the main difference between the two is whether you assume it is linear in that short distance, whether it goes straight out or whether it curves and goes straight out.

And I haven't heard any discussion that anybody has actually looked at that and addressed that. That's why statistically we can't help you on the two methods.

Now what you did is just a simple least square fits with a complicated model on one end. But that I don't have any problem with. That's probably the right thing to do. Changing the scale to a log scale, changes everything. I agree. I don't see any real justification for doing that at this point. You really don't have a lot of data to choose one or the other.

I would look at the residuals. How the heck am I going to tell between a normal and log normal with 12 observations. You haven't given me enough data to help me do that.

DR. REISS: I agree with Dr. Small's comments about how to do it. Just from practical experience it seems to work best the way you described. I was just wondering could you explain the physical reason why the log normal distribution changes the mass balance?

DR. SMALL: In my write up to question three I have a little bit of that, that sort of basically says that in your basic assumption in your atmospheric dispersion model is a linear superposition. That if you double the emission, you double the concentration.

If you take a log transform that no longer occurs, you are fitting a different relationship that's inconsistent with the underlying physical model -- fate transport model that you are using to get your explanatory variable which is the source receptor transfer coefficient.

So you have to be consistent with that underlying input to your statistical model.

DR. REISS: Thanks, that's clear.

DR. ROBERTS: Dr. Spicer?

DR. SPICER: I would like to comment a minute. There are a couple of data sets that I have gone through the exercise of taking the experimental concentrations measured horizontally and vertically and determining the dispersion coefficients from them and then going through the task of trying to close the material balance.

Now there were sufficient measurements in both those cases to do that effectively. There were nitrogen tetroxide tests and the kit fox carbon dioxide test.

I think that's why I made the suggestion that the vertical concentration measurements are extremely important.

And the comment that I made yesterday afternoon, I believe that's why what you are seeing when you compare the predicted concentrations with the observed concentrations, you have some observed concentrations that are non zero when the model says they should be zero.

Now, part of the reason why that occurs is when you get ready to run ISC, you have got -- although the wind speed can vary as continuous variable, the temperatures can vary as a continuous variable, the stability class is a step function. It goes from S stability to E to D, et cetera.

The point is that those coefficients then are not continuous functions, because they are dictated by the stability that you choose when you make those calculations.

Now what I found, of course, when I fit the data into 04 and the CO2 data was that the coefficients were not exactly what I would have calculated given the stability and the best estimates of stability. They were consistent but not exactly the same.

So the point is that by using the predictive dispersion coefficients you cannot expect to exactly fit the data. And, in fact, even changes, slight changes in elevation, because of the fact that you are talking about an area source on the ground, can make a significant difference in the concentrations.

And so I mean, just a seat of the pants answer, how might you be able to address the present data that you have in a more effective fashion, the only thing I can think is in somehow in ISC, instead of having the release at ground level, vary that level of the release and see if you get a better fit for the concentration profiles.

That way you might actually be able to recover some of that information. But you are still faced with that fundamental problem of the fact that when you tell

ISC to do calculation it is going to do it on the stability class you specify.

The dispersion coefficients are a continuous function of the stability parameters. They don't have this step -- in reality they don't have the stepwise behavior.

So it is very difficult to do much more than what has already been done.

DR. ROBERTS: Yes?

DR. SEGAWA: I have a question for Dr. Spicer. Do you have a recommendation on number and range of heights that the sampling should be done at?

DR. SPICER: That sort of design -- sure, that sort of recommendation can be made. I think that obviously what you would need to do is look at prevailing met conditions for the field and then look at how well your instruments will measure concentration, and how often you want to sample them. It is a nontrivial task but it certainly can be done.

DR. ROBERTS: Dr. Baker?

DR. BAKER: I had a question about the field studies injection method, the laying of the tarp, the possibility of potential for migration of the fumigant in the soil, such events that might cause a small lingering concentration at a receptor point where ISC everything has to move in one direction, downstream. And so the potential that maybe some of these low values where ISC is saying zero zero are due to just practical limitations of handling the material, applying the material, laying down the tarp, et cetera.

DR. REISS: It's possible.

I have been talking to a few people about this. And the likely reason you have some small concentrations that these receptors where ISC is predicting zero is particularly during low wind speeds. You have variable wind direction. So just for a few seconds or a few minutes the wind direction might be reversed from the predominant direction.

And because ISC averages that over an hour and then disperses it from there, that's probably the reason. I think from a scientific standpoint that's interesting. And there could be some other kinds of puff models that could deal with that.

But from a risk assessment standpoint the fact that we're getting that maximum concentration, estimating that well is the major goal.

DR. ROBERTS: Dr. Wang?

DR. WANG: My experience working with methylbromide is that degradation process is very simple. It is just raised to bromide iron. I assume for methyl iodide has a similar pathway.

I just wonder if that's something that you may be able to do to look at the increasing iodide irons and close up the loop on mass balances and taking soil samples. Is that something you have considered?

DR. REISS: I believe -- can you answer that Jim? I think it is something we have considered and addressed in the field dissipation studies. It is not something that I have done. It is part of the model, but it is something I think we have looked at.

DR. PLATT: We looked at that early on but not as part of the gas sampling. But in terms of the soil dissipation studies we sampled for both the parent and for the iodide and tracked those. But that's the only place that we have used them.

DR. ROBERTS: Let me get back to the Agency follow-up questions. I want to be sure that you guys have -- if there are any other questions that came up during the discussion over the last two days that you want to take advantage of the expertise sitting around the table?

MR. DAWSON: No. I think we have covered at all the topics that we needed to.

DR. ROBERTS: Great. Last chance for panel members to make comments before we adjourn this session. All right.

Let me, then, thank the panel members for their excellent preparation and discussion over the last two days on this topic and also to the Agency for their -- and particularly to Dr. Reiss for his presentation of the model and long discussions yesterday morning. That really helped the panel gain an appreciation for how the model works and the case study.

As always I would like to extend my appreciation also to the SAP staff for putting the panel together, getting everybody here, getting the materials here. It is quite a bit of work that goes on behind the scenes. They often don't get credit for that. I would like to extend my thanks to them for that.

Ms. Christian, as the DFO, do you have any announcement or anything you would like to say?

MS. CHRISTIAN: No announcements.

DR. ROBERTS: Yes?

DR. METZGER: Mike Metzger, EPA, over the past couple of days as I have been listening to this discussion, I have been focusing less on the intricacies of the science and more on how the information that you all have provided would be useful for us in making regulatory decisions and making good regulatory decisions.

And I have gotten 8 pages and 41 points down here. And I realize that you have had relatively a short amount of time to look at all this information and provide feedback for us. I just want to express our appreciation for doing this.

And to let you know that I do think the information, the data, the ideas that you have provided will allow us to make a better regulatory decision considering both our need to protect public health and to put the minimal burden on agriculture that we can.

DR. ROBERTS: Thank you very much. I neglected to thank our public commentors. I wanted to do that. We always appreciate the time and effort that people expend to come to the panel meetings and express their viewpoints and give us information.

That's a very important part of the process. If there is no other business to conduct on this particular session, this session is now adjourned.

There will be another session that begins tomorrow morning at 8:30, if I'm correct. Dr. Heeringa will be Chair. I would like to ask the panel members to meet i a short session now in our meeting room so that we can discuss preparation of the minutes for this meeting.

Thank you, very much.

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[Whereupon, at 3:15 p.m., the meeting recessed.]

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I N V O I C E

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